DISCOVERY

Monthly Notebook
Feeding the Golden Goose
Heavy Chemical Industry
Chemistry in America

British Association at Edinburgh
(Presidential addresses)

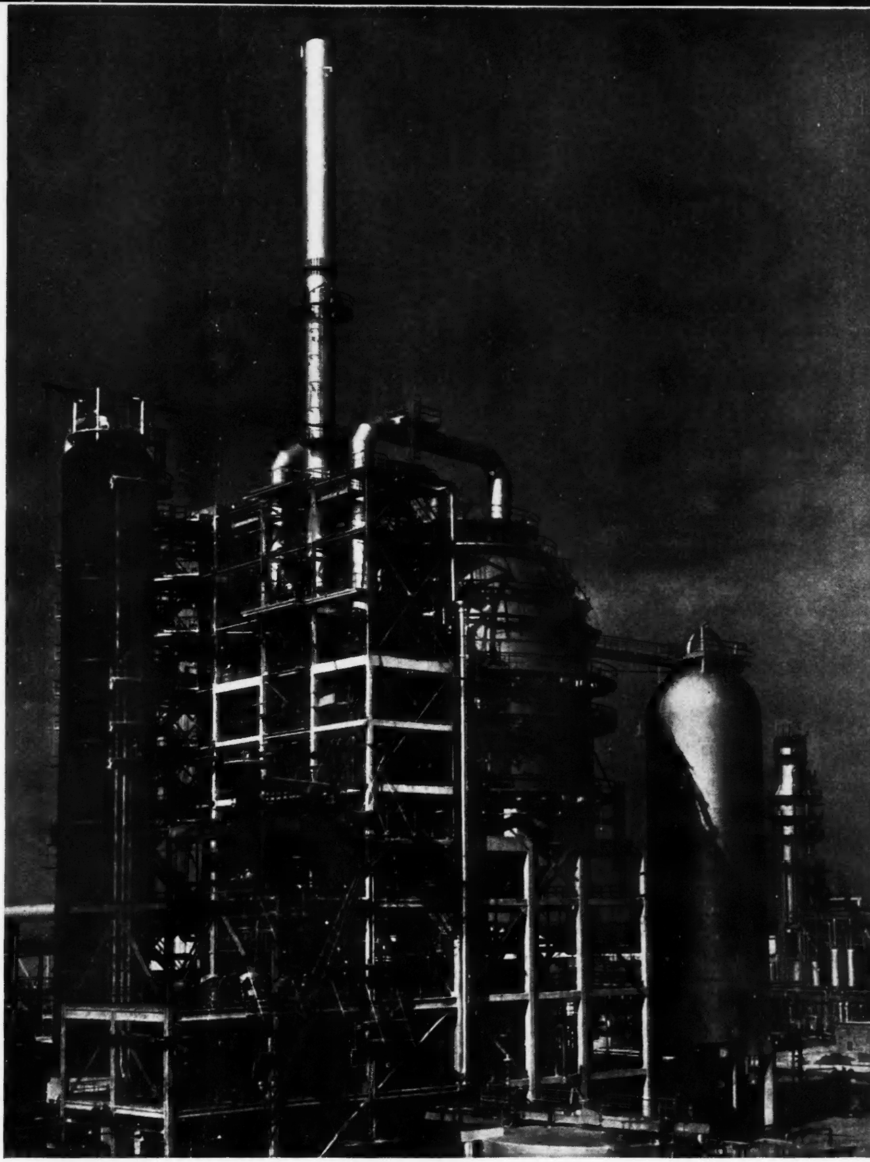
Genetics and and the World's Food
Prof. S. C. HARLAND, F.R.S.

La Caille: 10,000 Stars in Two Years
DAVID S. EVANS, M.A., Ph.D.

Ethics and the Technical Revolution
Prof. V. A. DEMANT

The Cloud Chamber
C. L. BOLTZ

Far and Near



Britain's first catalytic cracker went on stream last month at the new Esso Refinery at Fawley, which overlooks Southampton Water. Its capacity is one million gallons of high-octane motor spirit a day.

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The Progress of Science

Feeding the Golden Goose

REVOLUTIONARY ADVANCES in applied science and technology are based on advances made in pure science. In a journal like DISCOVERY this can be stated as an axiom with which all our readers are familiar, and there is no need to enlarge upon it. All over the world governments have adopted policies intended to encourage fundamental research as well as applied research, so that superficially it might appear that the axiom just stated has become a tenet of the legislators and the administrators.

Unfortunately the axiom is still not thoroughly understood outside the world of science, with the result that some measures that are taken with the good intention of fostering science both pure and applied end up by upsetting the balance between the two. In Britain, for instance, increased government expenditure on scientific research since 1945 has certainly increased the size of the scientific Civil Service and improved the status of the individual scientist who works in a government laboratory, but one cannot help wondering whether this has not been accompanied by a falling-away in the fundamental research effort in this country. Something similar seems to have happened in America, with the result that Dr. James B. Conant, the distinguished organic chemist who is president of Harvard University can write as follows: *I must confess to grave doubts of the wisdom of the expansion of governmental laboratories on the scale of the last ten years. I question whether the record supports any contention that a government laboratory is a favourable spot for fundamental investigations; furthermore, applied research looking toward industrial development can be best fostered by industry itself.*

This view is expressed by Dr. Conant in his latest book, *Science and Common Sense* (Yale University Press, 1951, pp. 371: published in Britain by Oxford University Press at 21*s*.). Dr. Conant is much concerned about the state of fundamental research in the United States, and the same attitude characterises the book entitled *The Neglect of Science* written by Prof. F. E. Simon of the Clarendon Laboratory, Oxford, and published by Basil Blackwell at 8*s*. 6*d*. The two books which readers will find well worth reading, can with advantage be considered together.

Both authors recognise that a first requisite for the fundamental research worker is absolute freedom for the individual to wander wherever conceptual ideas and observational clues may lead him; the quality of that freedom must be such that he feels no consciousness of a need to consider whether something that has captured his interest is relevant or not to his general line of research. This ideal freedom was available to the amateur scientist of a century ago: he was as free as the wind to choose from day to day the subject on which to focus his intellectual energy.

Modern research needs expensive equipment, and as the cost of research has risen so the question of how to raise the cash for his research has loomed larger and larger in the scientist's scheme of things. To impress organisations which can grant him facilities for his research, the scientist has been forced to draw up detailed paper plans, and very often the grant concerned is given for reasons other than the fact that the scientist is capable of doing first-class research.

Conant considers that committal to any programme is a handicap for anyone engaged in fundamental research. It is often argued that planning of the general direction in which a particular research unit should work would not really affect the freedom of the scientist to follow up any chance discovery or any clue which seems to lead away from the main track. It is argued that theoretically any good scientist should be able to follow up such accidental discovery, such as that of antibiosis due to penicillin, which Fleming observed.

On this point Conant is quite emphatic:

> *It is all very well to say that an able man even if committed to a programme of research will quickly turn aside to follow a promising new clue into other fields.* **But the record seems to indicate the contrary.**

In support of this view Conant quotes one very interesting case from the history of the researches leading to the discovery of the rare gases. As long ago as 1785 Cavendish had shown that air contains some inert gas accounting for "not more than $\frac{1}{120}$th part of the whole". Next step in the story was the discovery made in 1868 of a new element in the chromosphere of the Sun: this discovery

depended on the detection of a line in the sun's spectrum which came close to the yellow lines of sodium but which, however, did not correspond to any line given by an element known to be present on earth. The element was called helium, the presence of which could henceforward be detected by means of the spectroscope.

In 1889 W. F. Hillebrand was carrying out a systematic examination of certain uranium minerals in the U.S. Bureau of Mines, and on treating one particular mineral (cleveite) with acid he noticed that considerable amounts of a gas were liberated. This he collected and tested. His tests showed that it was not nitrogen. Moreover when he and his colleague, Dr. Hallock, examined the gas spectroscopically they noticed that the spectrum contained lines which "I could not identify with any mapped lines". They even went so far as to suggest (though in "a doubtfully serious spirit", wrote Hillebrand some years after the event) that "a new element might be in question". But there he let the matter rest.

In 1894 Ramsay isolated argon from air. It was then suggested to Ramsay that it might be worth testing the gas released when cleveite is heated with acid, as this might contain argon. Ramsay followed up this suggestion; he found no trace of argon, but discovered instead that the gas was in fact helium, which hitherto had gone undetected on the earth. Helium was later detected in the atmosphere, and Ramsay then went on to discover the presence of three more inert gases—neon, krypton and xenon—in the air.

Hillebrand's researches had thrown up a strong clue which suggested the presence of a new element in cleveite. Yet it was left to Ramsay to discover that this element was helium. Conant considers that it was the difference in their conditions of work that accounts for the fact that Hillebrand failed where Ramsay succeeded. Ramsay had the fullest freedom that a university scientist can have; he was to use Conant's term "an uncommitted investigator", whereas Hillebrand was committed to a definite programme, being head of a government bureau, the U.S. Geological Survey. Conant quotes Hillebrand's own frank admission about his failure,* which states quite categorically that "The circumstances and conditions under which my work was done were unfavourable; the chemical investigation had consumed a vast amount of time and I felt strong scruples about taking more from regular routine work".

Both Conant and Simon assume that it is essential to free the fundamental research worker from regular routine work, and they agree that today it is the universities which provide the best conditions for fundamental research. A university, in Conant's words, is a community of scholars whose aim is teaching and the advancement of knowledge. The member of a university has his obligations: these are, according to Conant, the obligation to perform his share of the teaching service and the moral obligation to advance knowledge *as he sees best*. "This means", says Conant, "that if he runs into a blind alley in his thinking and gets stuck there for a year, a decade, or a lifetime it is his own affair. His partners, the other permanent members of the faculty, will regret this sterility, but nothing can or should be done about it. And since his obligation actual and moral is dual he may take comfort in the success of his teaching. More than one unsuccessful (or unlucky) investigator has compensated in this way for his deficiency. That this is possible is one of the reasons why universities are still the only centres where a man interested exclusively in pure science can be fairly certain of finding in one way or another a satisfactory life."

Prof. Simon points to other important factors which make for the concentration of fundamental research in the universities. He expresses the view that pure research ought to be left to the universities because advanced instruction in fields in which the frontiers of science are advancing rapidly can only be given by a man who is himself actively engaged in research. Conversely the preparation of advanced lectures, and the necessity which this entails in the way of surveying all the literature on his subject, often clarifies the research worker's ideas and gives him new ones. Moreover the primary selection of the next generation of research workers is something that has to be done in the university, and this is another reason why teaching and research must not be separated.

Says Prof. Simon, "If they are separated the selection of research students has to be based entirely on the results of examinations and examinations are at present not designed for this, but for the selection of civil servants and teachers."

Prof. Simon devotes the greater part of one chapter to considering ways in which the freedom of the university scientist to advance knowledge can be increased. He finds that the most serious fault with the present university arrangements is that many scientists, in particular the younger scientists, may have to devote too much time to teaching. He suggests that university science faculties should be organised so that the individual scientist should be able to devote three-quarters of his time to research, and a quarter to teaching. Prof. Simon stresses the need to take from the shoulders of the university scientist all those tasks which are not relevant to his work. He regrets, for instance, the way time is wasted on excessive administrative and committee work; some of this may be unavoidable but Prof. Simon adds that scientists should certainly not be expected to do the work of secretaries. Scientific talent is wasted, too, because of the lack of skilled laboratory technicians. During the war science had little difficulty in securing money and equipment, and thus it became possible to figure out the cost of organising a laboratory with the right balance between senior and junior research workers, laboratory assistants, mechanics and clerical help. Prof. Simon sets a figure of about £1200 to £1500 a year *per research worker*. (This figure, of course, excludes any allowance for the purchase of exceptional equipment in the cyclotron category, for instance.) He says very emphatically that any laboratory not getting money at this rate is simply not making the best use of those rare birds, the first-class research scientists, and at the present time we just cannot afford this. Incidentally the corresponding figure for research laboratories in industry is, on the average, about three times as high, mainly because each research man needs more helpers to carry out the greater amount of routine work involved.

* It is worth noting that the discovery of helium, now of considerable economic importance in the United States, would have been a feather in the cap of any person like Hillebrand who was concerned with economic geology.

Prof. Simon does not ask for fancy salaries for university scientists. He sees no need for the salaries to be competitive with those paid in industry ("university scientists will gladly put up with smaller financial reward for their increased freedom to choose and to publish their work"), but they must receive enough to make ends meet. Prof. Simon quotes the experience of France in this connexion, for the major reason for the decline of French science has been the poor payment of her scientists—so poor is it in fact that professors often have to take on two or three jobs, with disastrous consequences all round.

University research requires millions of pounds per year, and only governments can find money on this scale. Prof. Simon points out that the government that pays over the money must always be prepared to forego the privilege of calling the tune.

The danger which arises because governments have to meet bills for university research, and therefore feel entitled to try to influence the directions of research has not been avoided in every country, but Prof. Simon finds that the position in Britain is satisfactory. "There are no 'strings' attached to the money distributed by the University Grants Committee and the Department of Scientific and Industrial Research", which are the two main instruments for distributing government money to the universities. "Without these grants research would come to a standstill and one can say that everyone who gives reasonable promise of becoming a good research scientist can obtain a research grant for two or three years."

The support of research at immediate post-graduate stage Prof. Simon finds adequate but he expresses doubt whether there is anything like adequate support for post-Ph.D. research. He suggests that industrial firms could help here; indeed he thinks that more money from industry would make for a healthy state of affairs for he considers that it is good for the universities to have more than one string to their bow. (This view is expressed by Conant in these words: "The future of the uncommitted investigator is best assured by providing him with a bow of several strings; and only one of these should be money voted by the Congress of the United States.") Such a development would correct biases that creep in because of the universities' excessive reliance on government money; it would correct, for instance, the present over-emphasis on nuclear physics.

Once a scientist has received a grant, it is only to be expected that he will receive some guidance from the head of his department. There may be too much guidance, sometimes too little.

As Prof. Simon says, in Germany the tendency has been for the laboratory to exist more or less to add to the glory of the professor; in France, and often too in America, the tendency has been for a laboratory to be split up into too many independent research units. Prof. Simon thinks that the best balance perhaps has been struck in Britain, "where people are individualists but where nevertheless the existence of a team spirit prevents the laboratory from breaking up into too small units. This is probably one of the reasons for the high standard of fundamental research in this country."

Prof. Simon wants to see more money made available to facilitate travel by scientists. He thinks that the British scientist has far too few opportunities of meeting his foreign colleagues, and considers this to be particularly true for the younger scientists, who would benefit most from the stimulus of meeting foreign scientists. Says Prof. Simon, "It is astonishing in these times when everyone speaks of building a unified world, or at least a united Europe, that no more thought is put into this matter. It is pathetic to see what obstacles have to be overcome in order to finance even a short visit to, say, Holland or France, or to invite a Dutch or French scientist to lecture in this country." Prof. Simon refers to the help given by the Royal Society and the British Council here, but says that these bodies can only offer help on a scale much too small. Prof. Simon thinks that the larger university laboratories ought to have every year several hundred pounds each to spend on this item, and that there should be a central fund for the benefit of scientists from smaller laboratories. This matter does seem to be calling for early action, not only by the British Government and by the British Foreign Office, but also by organisations such as Unesco and the Council of Europe.

Sir William Ramsay, whose 'uncommitted' researches led to the discovery of a whole new family of elements—the rare gases. (*This drawing by W. Kassemoff is reproduced from "Ancestors of an Industry"*.)

Heavy Chemical Industry

WHEN DISCOVERY ("Progress of Science", May 1951) gave the news that Britain's shortage of sulphuric acid would be overcome by building a plant to exploit the anhydrite process, the site of the project had not been decided. The

announcement has now been made that the new works will be built in Widnes and will absorb the output of a new anhydrite mine in Scotland. This plant will be run by the British Plaster Board Company (for which the mineral anhydrite is a normal raw material); the cement produced as a by-product will go to a new works in Widnes of the Associated Portland Cement Co., while the 150,000 tons of acid a year, representing nearly 10% of the country's needs, will go to eleven firms which use this acid and are putting up the capital for the plant.

The news will be received with sober satisfaction in Widnes. There is not much romantic about the spot, beyond what romance there is in the story of the development of the heavy chemical industry. This has now been recorded by Dr. Hardie* on behalf of the General Chemicals Division of I.C.I., and when one reads the history of the heavy chemical industry in Widnes one begins to sense the exciting and turbulent development that has occurred there. One begins, too, to understand the attachment of natives and off-comers to the place which has been immortalised by Stanley Holloway in his song about Runcorn transporter bridge. (The full pathos of "tuppence per person per trip" can only be felt by those who have queued up at 5.30 p.m. on a bleak and wet evening, looking across the Ship Canal and the Mersey to the lights of Widnes and waiting patiently for their turn on that crazy platform.)

The story of Widnes is the history of the Industrial Revolution told in terms of industrial chemical development. It can be dated back to 1847 when a young chemist, John Hutchinson, set out to build the first alkali works in Widnes in the company of Thomas Robinson, the iron-founder. The alkali produced was, of course, sodium carbonate, which had become so important by that date as the raw material for soap manufacture. Large quantities of soap were required by the growth of the textile industries and the traditional sources of soda, such as the ashes of seaweed and of the glasswort, (*Salsola kali*), were no longer adequate.

LeBlanc had invented in 1791 the soda ash process whereby salt was decomposed by sulphuric acid to give sodium sulphate (saltcake) and hydrochloric acid. The sodium sulphate that resulted was reduced, by roasting it with coal, to sodium sulphide which on heating in the presence of limestone became converted into sodium carbonate. The credit for introducing the process to England belongs to William Losh, who visited Paris in 1802 and set up a works in Walker-on-Tyne. Subsequently similar works sprang up in St. Helens and Liverpool. The new product had a struggle to overcome the conservatism of the soap-boilers—and the objections of neighbours to the works. Gossage described the scene at Muspratt's soda works in Liverpool (set up in 1823) as follows: "He took common salt, put it into a furnace made of brickwork, a reverberatory furnace, so arranged that it had a fire at one end and a communication with a chimney at the other: he laid the salt on the floor of the furnace: he then ran upon it sulphuric acid and an immediate decomposition took place, forming sulphate of soda, in which decomposition an immense evolution of muriatic acid took place: that passed through the flues up the chimney and into the atmosphere; and as muriatic acid is heavier than air, in a short time it came down on the surrounding neighbourhood." When Gossage started his own alkali works in Worcestershire seven years later, he got over this trouble by adding a pioneer piece of chemical engineering—an absorption tower. This tower was actually a derelict windmill which Gossage filled with gorse and brushwood, then irrigating this with water and passing the gases through it.

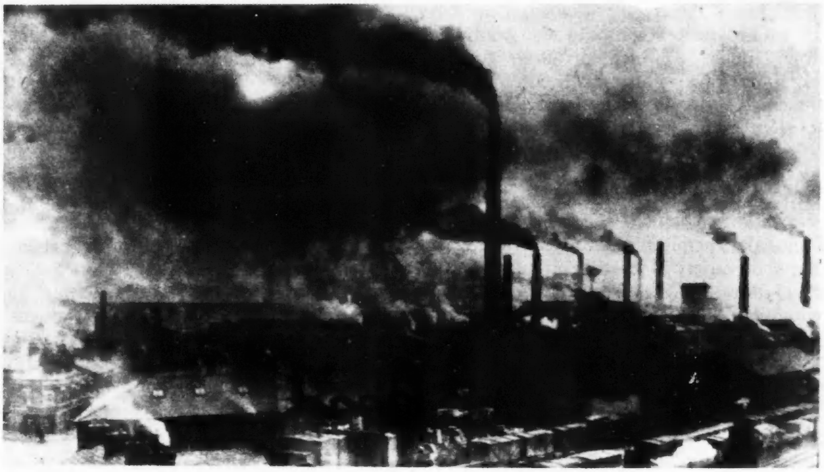
Muspratt pinned his faith on taller and taller chimneys, one of them being as much as 276 feet high. The newly established manufacturers of Widnes were too busy with quick returns to bother about the nuisances they created such as atmospheric pollution, and to the fumes of hydrochloric acid more noxious chemicals were soon added; for instance, sulphuretted hydrogen, which was liberated from the waste heaps of calcium sulphide left to rot away in contact with air. By 1862 a select committee of the House of Lords was appointed to inquire into these nuisances, and the upshot was the appointment of Alkali Inspectors who began their series of famous reports in 1864.

Another result was an invasion. Ludwig Mond, a young German, sailed for England in 1862 with a process for oxidising the waste sulphide in air and extracting with water, then acidifying the extract and precipitating sulphur. It was not for another twenty years that a satisfactory method was to be worked out by Chance in time to bolster up the LeBlanc process. Mond's lack of success in persuading the alkali manufacturers to adopt his schemes finally led him to enter into competition with them, and he started to operate the ammonia-soda process which Solvay had patented in 1872. The only by-product of this process was innocuous calcium chloride, and it was economic given cheap ammonia and a satisfactory method for recovering the ammonia. It was a hard road that Brunner and Mond had to tread after their original purchase of Winnington Hall and 130 acres in the Cheshire salt fields for £26,000—it was in fact the confidence of British bankers as much as anything else that saw them through their trials and tribulations.

Widnes still turned its back on the better process of alkali manufacture, and by 1891 it had buried five hundred acres under a layer of alkali waste, twelve feet deep. To a sulphur-hungry world, the comment of the alkali inspector in that year has a sadly ironic ring: "Ten million tons lie there, all of which contained, when fresh, 15% of sulphur in an available form."

The story of Widnes becomes subsequently one of amalgamations and new processes. The threatened LeBlanc manufacturers saved themselves by combining to form the United Alkali Company in 1890 and by changing over to the ammonia-soda process. They also centralised their research, rationalised their production and proceeded to pioneer the use of chlorine and its derivatives. By the 1914–18 war, Widnes was the site for electrolytic production of chlorine, and it was Widnes that saw the introduction to Britain of plants to make sulphuric acid by the 'contact' process—that is, the oxidation of sulphur dioxide over platinum catalyst. (As mentioned in our last issue, on p. 292, the first 'contact' plant was in the Tentelev chemical factory in St. Petersburg.) In its early days the United Alkali Company depended on its chief chemist, Frederick Hunter, prototype of the great industrial

* _A History of the Chemical Industry in Widnes_, by Dr. D. W. F. Hardie, I.C.I. Ltd., General Chemicals Division.



The smoke pall over Widnes in the heyday of the LeBlanc process, when the chemical works were consuming over a million tons of coal each year. In this century the chemical engineer has brought about a revolution; pollution of air and water is now regarded as synonymous with wastage of raw materials, and modern chemical factories do not spoil the amenities of their neighbourhoods.

chemists of modern times. Dr. Hardie's book must be read for Hunter's importance to be appreciated. In fact the whole book has a great deal to say about great men. There was, for instance, Hargreaves, who originated the Society of Chemical Industry along with others such as George E. Davies, who is generally regarded as the father of chemical engineering.

So we come to modern Widnes, the site of 'Tube alloys' which did the fundamental work on uranium compounds which went into the American atomic bomb project, where Gammexane was discovered and made, and where new plastics like polyvinyl chloride and polytetrafluoro-ethylene are made. It was also the centre for the amalgamation of chemical companies which gave us Imperial Chemical Industries, and it was the birthplace in 1868 of Alfred Mond, later Lord Melchett. Fifty years from prose to poetry: from father just getting by with the help of the Westminster Bank to the time when T. S. Eliot could write (in 1920, to be exact):

I shall not want Capital in Heaven
For I shall meet Sir Alfred Mond;
We two shall lie together, lapt
In a five per cent Exchequer Bond.

Chemistry in America

In the last 75 years, the *modern* chemical industry has come into existence in Britain. Before about 1875 chemical production here was virtually confined to a few 'heavy chemicals' like sulphuric acid and caustic soda. The industry which existed in America in 1875 was much less mature than Britain's, and so a review of American chemistry over the past three-quarters of a century covers the development of almost the whole of America's chemical industry. Such a review was provided by the meeting last month at which the American Chemical Society celebrated its 75th anniversary, and it yielded most useful information about the great increase in scale and importance of the U.S. chemical industry which has occurred during the lifetime of the society. This subject is of much more than local interest, for it exhibits features of universal significance.

The first meeting of the American Chemical Society was held on April 6, 1876. Since that day momentous changes have occurred in the United States, and progress in chemistry accounts for many of those changes. It will be difficult for most readers to grasp the difference between life in 1876 and life today, and so it is worth quoting the words of Herbert Hoover, only living ex-President of the U.S.A. and a trained engineer by profession, whose life covers the whole of the period since the A.C.S. came into existence. He was born in Iowa in 1874—the same year that a group of chemists first proposed the formation of the American Chemical Society. He describes life in his home town in these words: "Iowa of those days was not without its tragedies. Medical science was still almost powerless against the contagious diseases which swept the countryside. My own parents were among their victims. My father succumbed to typhoid fever; my mother when I was eight, to pneumonia. At that stage in the agricultural history of Cedar County, a farm was not only a farm but all kinds of factories. Here the family performed all the functions of a Chicago packer, a Cincinnati soap company, a Duluth carpet factory and a California canner. Every fall the cellar was filled with bins and jars and barrels. That was social security itself. The farm families were their own lawyers, labor leaders, engineers, doctors, tailors, dressmakers and beauty-parlor artists. They developed high art in feathers and wax. My clothes, partly homespun and dyed with butternuts, showed no influence of Paris or London. As gentle as are my memories of those times, I am not recommending a return to the good old days. Sickness was greater and death came sooner. While the standards of living in food and clothing and shelter were high enough for anybody's health and comfort, there was but little resource left for the other purposes of living."

Today the three industries of soap-making, carpet manufacture and food processing are important chemical-processing or chemical-consuming industries. One is reminded by Hoover's indirect reference to dyestuffs that the dyestuff industry has become one of the most influential

branches of the chemical industry, not merely because of its immediate products but also because it provided the foundations on which has been built the much more comprehensive industry concerned with the synthesis of organic chemicals. His reference to disease is a reminder of the startling progress made in chemotherapy.

An impression of the vast strides industrial chemistry has made in the lifetime of Mr. Hoover and of the A.C.S. is conveyed by merely listing a few of the items which we now take for granted and which we owe to the efforts of industrial chemists: such items as synthetic rubber, rayon, the wide variety of plastics, no-knock gasoline, chlorinated (and now fluorinated) water supply, foods enriched in vitamins, DDT, foam rubber, reliable fire extinguishers, penicillin, pocket lighters, waterproof nail polish, aviation gasoline, cellulose sponges, plastic dentures, 'horn-rimmed' sun glasses, the refrigerants, without which electric and gas refrigerators would not be feasible, dry ice, frozen foods, anti-freeze, motion-picture film, stainless steel, non-inflammable fibreglass cloth, kissproof lipstick, air conditioning, painless dentistry, contact lenses, light-weight hearing aids, synthetic bristles, cheap and unbreakable gramophone records, colour photography, blood extenders, insulin, chemical fertilisers, household dyes, quick-drying enamels and synthetic lacquers, modern sewage disposal —and bubble gum.

On the less material side progress in chemistry in the last 75 years has brought with it a much more detailed understanding of the nature of the universe, of the life processes of plants and animals, and of the elements. This progress has extended the boundaries of human knowledge and the depth of our awareness and comprehension. Pure chemistry is today an indispensable element in modern culture. The chemist has added to the range of colours available to the artist, and he has played an important part in the development of the most important new art form of this century, the cinema. The gramophone record, as much a result of chemical research as is cinema film, has brought a wider appreciation of music—and it might be whispered that modern broadcasting could not continue to exist without it!

The early years of the A.C.S. were by no means easy ones, and indeed its progress was at times painful. For some years its activities were restricted to New York City, where all A.C.S. meetings were held up till 1890. In those days chemical education in the U.S.A. was only beginning to find its feet, so that many U.S. chemists had to go to Europe for their training, particularly at post-graduate level. Many of the pioneers in chemistry and chemical engineering came from abroad in the successive waves of immigration.

In the field of applied science, dependence on European nations (in particular, Germany) was even greater. The Germans took full and unscrupulous advantage of this fact. Long before the outbreak of World War I Germany embarked on a campaign of economic warfare with the object of reducing the war potential of industries inside all countries that might become potential enemies once she started open warfare aimed at military domination of the world. She did, in fact, succeed in crippling key sectors of the U.S. chemical industry. To prevent the U.S.A. from manufacturing dyestuffs, she put out of business the small firms which had started producing such materials by systematically undercutting them. Dyes that cost, for instance, 22 cents a pound in Europe were sold by the Germans at 17 cents and less in America. The German firms also took out patents which completely blocked the manufacture of many chemicals, in spite of the fact that many of these patents were such deliberate frauds (in that they left out key details of manufacture) that the processes described were not rightly entitled to patent protection at all. Where the Germans met no U.S. competition, they overcharged for their chemicals—thus aspirin cost 35 cents an ounce to America, though its price in Germany was only 2 cents. Certain substances, for instance methyl violet, which the Germans sold in America, were heavily adulterated with cheap substances such as starch. By such methods the German chemical industry, which enjoyed many favours from the German Government (such as subsidies for research and preferential rates on the State railways), was able to reduce the war potential of the U.S. chemical industry. For example, when war broke out only seven small dyestuff companies remained in production in the U.S.A. There was, moreover, "an American famine in all coal-tar chemicals", as the Germans had smashed U.S. production of benzene, naphthalene and anthracene, as well as the production of finished dyestuffs and drugs. When war broke out America lacked all kinds of chemicals; so great indeed was her lack that America had practically no alternative but to accept the dyes and medicines that were brought through the British blockade by a German submarine!

The Americans virtually had to begin building their fine chemical industry from the ground up when they were left bereft of German imports in 1914. As is their custom, they set to work with a vengeance, and some idea of the rate of expansion of U.S. chemical production may be gained from these figures: at the end of 1915, the U.S. made less than 2 million gallons of benzene a month but three years later output had risen to 5 million; the output of toluene was increased from about half a million to nearly $1\frac{1}{2}$ million gallons a month in the same period.

Almost overnight the flow of scientific information from Germany about chemical products had ceased. It was fortunate that in this crisis there existed in the American Chemical Society a professional organisation which had accumulated all available data on chemical science and technical processes. Even so, there was a critical period of readjustment which lasted well into the post-war years. Chemical self-sufficiency was achieved between the two world wars, and no chemical famine hit America in 1939. So fast had been America's development on the chemical front that one finds, for example, that she was well able to cope with the many difficult problems, both pure and applied, that arose in connexion with the atomic energy project, and it is now recognised generally that America, *and only America*, could have solved all the relevant problems and produced an atomic bomb before the end of World War II.

Today the standing of American chemistry can be judged from the fact that the publications of the American Chemical Society are considered indispensable by chemists everywhere. There is the *Journal of the American Chemical Society* for pure research, while *Chemical Abstracts*, now

[*Continued on p. 314*

ON OCTOBER 7, Oxford University Press is publishing a book entitled *Forty Thousand Million Mouths*. This volume, edited by nutrition expert F. Le Gros Clark and N. W. Pirie, F.R.S., of Rothamsted, is the most comprehensive scientific document yet to appear dealing authoritatively with the problem of providing food to maintain the ever-increasing population of the world. The book provides a historical study of the long controversy on the relation between the human community and its food supply; it goes on to deal with many of the problems of conserving soil, breeding and manuring for higher yields, increasing the catch of fish and the yield of dairy herds, and preventing the enormous waste of our resources. All the essayists are authorities on their subjects and they keep the world problem in mind while writing in terms familiar to British readers. The following contribution by Prof. S. C. Harland, F.R.S., Professor of Botany at Manchester University, represents a typical chapter from *Forty Thousand Million Mouths*.

Genetics and the World's Food

PROF. S. C. HARLAND, F.R.S.

MANY writers have recently made our flesh creep discussing the three main perils which menace our present civilisation: the peril of a world population too large for the present land area to feed; the peril of soil erosion; and the peril of reckless destruction of forests and natural vegetation. These questions have been verbally chewed over or subjected to cerebral mastication so thoroughly that ordinary thinking folk in every part of the world are both intellectually alarmed and emotionally aroused by the gloomy prospects ahead.

But this will not of itself lead to a world policy in which the second and third perils could be promptly put an end to within the next fifty years, nor to a policy of optimum efficiency in the management of the deteriorating and shrinking lands left to us. It needs a lot more intelligence and what the maize breeders call combining ability than our species seems to have. I say this, because as a biologist through rather a long life of research, teaching, and travel, I have been more struck by the stupid things that mankind has done, is doing, and will no doubt continue to do in the future, than by the slow capillary creep of the fluid of intelligence up the blotting-paper of human inertia, cupidity, and ignorance. But it is in some sense a hopeful sign when the Colonel Blimps all over the world start saying, "By Gad, sir, Malthus was right" (as indeed I think he was). And it is a hopeful sign that our Government began to think on a continental, if not a planetary scale, when it fathered the groundnut scheme of Africa, which although certainly destined to be the biggest flop since the South Sea Bubble, will only fail because it was planned by the wrong men, accepted as sound by the wrong men, administered by the wrong men, and put in the wrong part of what may even be the wrong continent. The groundnut scheme is (or was) an experiment primarily in applied plant physiology and genetics, and if you want to be successful in this sort of game the rules of biology come first and the rules of engineering and administration come second. There is no evidence that the scheme was ever considered from the point of view of its fundamental biological bases, and it is hardly necessary to say that you can't obey the rules of biology unless you know what they are.

It is not my purpose, however, to deplore the lack of a biologically tinged outlook in our rulers, nor to stress my belief that in our thinking about the problem of world population and world food supply the lack of such an outlook prevents the swift, impetuous, and emotional approach which the situation demands. It is also not my purpose to say emphatically that whatever we do to raise the level of the world's food supply, there will be prompt human spawning up to that new level. My object is clear and definite. The world's food supply has got dangerously low; what can applied genetics do to increase it both quantitatively and qualitatively?

Let us first consider what primitive man did to domesticate a few thousand plants out of the several hundred thousand plant species available to him. If we do this we may be able to streamline the domestication process and domesticate some more. Or we may complete the domestication process in some plants which are at present only half-tamed.

Since the dawn of history man has been an ardent student of the plants and animals which formed part of his environment: and upon which he depended for food. He learned through long and often painful experiment which plants were good for food or which, such as tea, coffee, or tobacco, possessed unusual or attractive physiological properties or, such as ephedra or digitalis, had useful pharmacological properties. All primitive peoples are biologists. They possess a staggering amount of knowledge about their plants and animals, and any new plant which they encounter will undergo a battery of tests. Can it be eaten or used to prepare a drink (preferably alcoholic)? Has it any medicinal value for man or animals? Can it be used to poison anything that you want poisoned? I am reminded of two cases of this sort. In the north of Peru I found that a special variety of soya bean had been introduced and had become acclimatised. It was not used for food, however, but for making alcoholic liquor. In the same region I found the Okra plant, well known in other parts of the tropics as a palatable vegetable. Here again, its use as a food was not known, but the ground-up dry seeds were used to brew a drink faintly resembling coffee. The plant was known as 'café silvestre' or wild coffee.

In time, man turned from hunter and seeker of wild food plants to cultivator of the ground. He had already explored the dietary possibilities of several thousands of plants and he now began consciously and unconsciously to domesticate many of them. He did this by applying the principles of applied genetics, and by using methods which are still used today. He used Mendelism as M. Jourdain used prose—without knowing that he was using it—and changed many plants so drastically that the original species from which they were derived are now not known. This

is true, for example, of maize, which seems to have been domesticated in Peru or Bolivia perhaps more than 4000 years ago. How did man domesticate plants? He brought home edible tubers and seeds (some inside him) and he made great heaps of refuse outside the door which constituted a completely new type of ecological niche, enormously rich in nitrogen, phosphorus, and potassium. These heaps would be colonised by the discarded tubers and seeds of some of his wild food plants and also by rapidly growing weeds capable of using this kind of soil. It is fairly clear that some crop plants, e.g. potato, tomato, and maize, originated from what Anderson has called dung-heap superweeds. In this class may also be put the chenopodiums and amaranths of South America, the millets of Africa, and some other cereals. Some crop plants probably originated as weeds in an already domesticated plant, e.g. rye is thought to have originated as a weed in wheat patches.

When a plant was recognised as useful it would spread over wide areas and there would then arise the opportunity for extensive hybridisation not only within the species, but also with other species. New types would result from such hybrids, adapted to new ecological niches, and new crop plants would arise. Mendel himself believed that "our cultivated plants are members of varied hybrid series, whose further development in conformity with law is varied and interrupted by frequent crossings inter se. . . . Cultivated plants are mostly grown in great numbers and close together, affording the most favourable conditions for reciprocal fertilisation between the varieties present and the species itself."

To sum up: the origin of cultivated plants is intimately connected with (*a*) the selection by man through trial and error of wild edible plants, (*b*) the provision of new ecological niches enabling improved types to manifest themselves, and (*c*) the wide diffusion of cultivated plants providing opportunity for intra- and inter-specific crossing, leading to the emergence of new types, to which sometimes two or more species have contributed. A good example of a new species originating by a combination of all these ways is the bread wheat. This has been synthesised from three distinct species—a primitive wheat and two grasses. One of these grasses has been identified as *Aegilops squarrosa* L., and the other is not known with certainty but is probably another species of *Aegilops*.

Now if we wish to create new and more valuable plant forms we cannot do better than follow nature. We know pretty well how many of the cultivated plants have reached their present perfection. What are the perspectives for the future?

Is it possible that primitive man did not domesticate all the useful plants and that we can domesticate some entirely new ones by short cuts known to plant breeders?

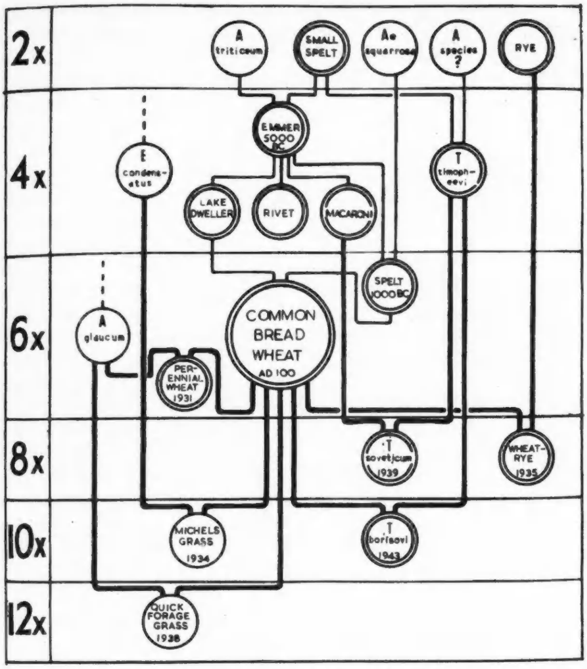
It is likely that many wild plants could be improved and thus be incorporated into human diet without much difficulty, but except in rare instances such plants would not have any advantage over those already cultivated.

The historic example of the domestication of a wild plant is of course the sugar-beet. More than a hundred years ago Louis Vilmorin of France worked on the selection of the sugar-beet for high sugar content. He threw the beets into a strong solution of brine. Most of them floated, but the sinkers were of greater specific gravity and presumably of higher sugar content. In 1851 chemistry had got so far that sugar content could be determined by chemical analysis, and was found to vary between 7 and 14%. Selection for high sugar content has continued ever since. But in spite of much progress through more than 100 years of study, improvement is still being sought. Crosses have been made on a large scale with other wild species in the genus and also with the ordinary red garden beet. The ordinary type produces seeds with several germs and this is a nuisance as the resulting clump of seedlings has to be thinned by hand. So varieties with single germ seeds have been produced. In most countries non-bolting varieties are being looked for; in California a cold-tolerant variety can be planted in October and reaped in May or June. New methods are being used to increase the already high sugar content and for resistance to various troublesome virus diseases. Here it may be said that the plant breeder never reaches a point at which he can say that it isn't worth while working on the plant any more. However many improvements have been made there are always others made possible by new methods and necessary by new demands from industry.

Here, then, is one example of the domestication of a wild plant which has taken place in our own times. Another good example is the recent domestication of the American blueberry by Coville and his successors. This fruit is not unlike the English bilberry. It has responded to selection so well that the latest varieties have fruits over an inch in diameter. The hawthorn and the elderberry could both probably be converted into valuable and palatable fruits, and some edible hawthorns are known in France.

But perhaps more possibilities for the future may lie in the further improvement of plants already partly domesticated. A plant cultivated for one purpose may have its usefulness greatly increased. The lupin is one example of this and the cotton plant is another. The European lupin has for centuries been used as a forage plant for sheep. The seeds contain a poisonous alkaloid which has hitherto prevented its being used as a human food. In Germany and the USSR it has been found possible to eliminate the alkaloid by selection and a new source of high-quality vegetable protein is now available not only for animal but also for human consumption. Another species of lupin, *Lupinus mutabilis*, is grown at high altitudes in Peru and is used as a human food. It is rather like wheat, in that probably more than one wild species has contributed to its make-up. The seeds also contain a poisonous principle, but the pre-Incas learned to wash the poison out by treatment with running water for a few days. They are then very palatable and contain more than 40% of good protein. German workers (Sengbusch and Zimmermann, 1947) have been able to breed alkaloid-free varieties and these could be used over an area of several million acres in the Andean region. This lupin is highly productive and will grow well on poor soils almost up to the snow line. It would serve both as animal and human food and would enable the sheep-carrying capacity of large areas in this region to be at least doubled. Here is an example of a plant which early man did partly domesticate, and which has been rendered vastly more useful by the plant breeder.

The production of a cactus without spines by Burbank



(*Left*)—The wheat used for making bread has been synthesised from three distinct species—a primitive wheat (*Triticum monococcum* or 'Small Spelt'), and two grasses, one of which was *Aegilops squarrosa*. All these species are diploids and possess 14 chromosomes. Their seeds were small; the primitive wheat stock, which occurs wild in S.W. Asia, has tiny grain—a hundred grains weigh no more than a gram. 'Spelt', the first hexaploid wheat with $6x$ (*i.e.* 42 chromosomes), probably originated in the Caucasus or Crimea. Because the ear is brittle, 'Spelt' does not thresh easily. The origin of the free-threshing quality of Bread Wheat is not certain, but a crossing between 'Spelt' and the tetraploid Lake Dweller wheat is indicated. (Original drawing by Dr. A. J. Bateman, DISCOVERY, 1949, p. 281.)

(*Right*)—The scientific synthesis of 'Spelt'. From left to right, ears of: $4x$ Emmer Wheat; $2x$ *Aegilops squarrosa*, wild grass in Middle East; the $6x$ synthetic hybrid between these two; $6x$ 'Spelt' is supposed to have arisen spontaneously in the same way.
(*All natural size. Reproduced from articles by McFadden and Sears in "Journal of Heredity"*, 1946.)

was a valuable first step in the domestication of the cacti. This group is one which responds rapidly to selection and improved varieties would be of great use in the arid regions of the world where little else will grow. Some improved varieties are already known in Mexico and Peru.

We come next to what may be called multi-purpose plants, of which a good example is the cotton plant. Cotton is grown primarily for the convoluted cellulose hairs that spring from the seed coat. The seeds used to be thrown away as useless, but it was then discovered that a valuable oil could be extracted from them and that the residue was a valuable protein food for most animals. The amount of oil in the seed varies a good deal according to the species and variety. American Upland cotton ordinarily has about 15–16%, and Peruvian Tanguis about 18–20%. While working in Peru the writer (Harland, 1945) began to select for high oil content and succeeded in producing a new variety with 29·4% of oil. Now this relatively simple selection experiment puts cotton into the category of a first-class oil plant. And the quality and quantity of the fibre are unaffected. So when other countries such as Egypt, the USA, and India go over to high oil varieties, it looks as if cotton will be the world's most valuable oil plant.

It has been shown in the USA that maize can be selected for high oil and high protein content. In the future all our agricultural plants will have to be chemically streamlined. Good amino-acids will be increased and toxic compounds reduced or eliminated. The vitamin content of most fruits and vegetables will be doubled or trebled. The palatability and nutritive value of forage plants will be augmented. There are hardly any limits to what can be done by breeding plants to engineering specifications.

One more example: the high-altitude maize of Peru has a stem so rich in sugar that children chew the stem as

negro children chew sugar-cane in the West Indies. Here, then, if we need it, is a new sugar plant which for many areas may be potentially better than the sugar-beet, or even the sugar-cane.

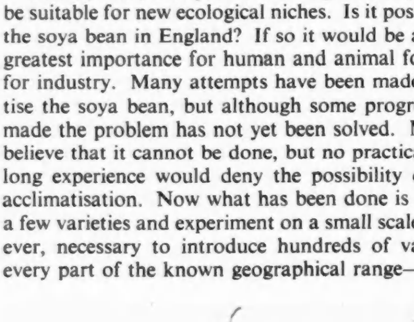
Another way of making already existing cultivated plants more useful is to fit them to new ecological niches, or to widen the climatic conditions under which they can be grown. The cultivation of outdoor tomatoes in this country is at present limited to favoured parts of the south of England. Until quite recently tomatoes were not regarded as an outdoor crop at all. Breeding for cold resistance has given us some valuable and productive outdoor tomatoes. Now a considerable northward extension of the tomato area is almost certainly possible. The writer once made a hybrid between the wild tomato of the coastal area of Peru and an American cultivated variety. The second generation of the cross was grown in Peru at an altitude of 10,300 feet above sea-level. The plants showed enormous diversity. Some had fruits only as big as small cherries; others had fruits weighing up to 500 grammes or more. The plot was left through two winters. Three years later it was found that two plants which had undergone more than 10 degrees of frost in two successive years were still flourishing—the basis of frost-proof varieties. In the same garden was a tobacco plant which had also survived for two years.

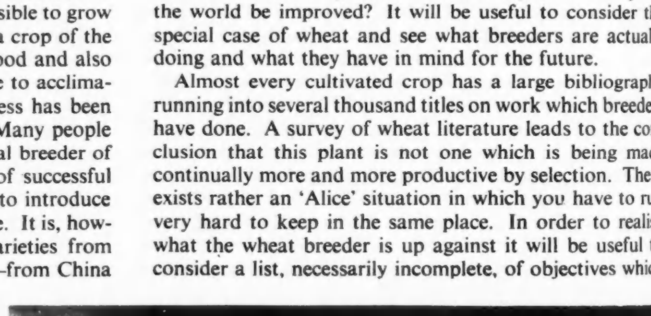
We see, then, that plants can be genetically moulded to be suitable for new ecological niches. Is it possible to grow the soya bean in England? If so it would be a crop of the greatest importance for human and animal food and also for industry. Many attempts have been made to acclimatise the soya bean, but although some progress has been made the problem has not yet been solved. Many people believe that it cannot be done, but no practical breeder of long experience would deny the possibility of successful acclimatisation. Now what has been done is to introduce a few varieties and experiment on a small scale. It is, however, necessary to introduce hundreds of varieties from every part of the known geographical range—from China to Peru—and also all the possible wild species with which the soya bean may cross. The next step is to make hybrids on a gigantic scale, and cross everything with everything else. Then begin in the south of France and plant the whole hybrid mixture at intervals of 50 miles in a northerly direction as far as the latitude of south Sweden. Study the morphology and physiology of the crop at each locality and find the point at which the crop begins to be climatically maladapted. Go on in that area with material even more heterogeneous and make more hybrids with the best types. Give the material time to settle down in this area and then slowly move north again. It would be a good thing also to grow the whole heterogeneous collection of soya beans in constant-temperature chambers and reduce the temperature by half a degree every generation. What degree of adaptation to cold would emerge is not known but what every geneticist does know is that experimentation on a less ambitious scale will achieve little or nothing.

Some other countries have done this sort of large-scale work, though not always with the right point of view. But whenever spectacular success has resulted it has always been due to precise and specific objectives using very large populations. Baur in Germany worked on the transfer of resistance to Phylloxera and mildew from the wild American grape to the European grape. He worked with millions of seedlings.

In what other ways can the main staple food crops of the world be improved? It will be useful to consider the special case of wheat and see what breeders are actually doing and what they have in mind for the future.

Almost every cultivated crop has a large bibliography running into several thousand titles on work which breeders have done. A survey of wheat literature leads to the conclusion that this plant is not one which is being made continually more and more productive by selection. There exists rather an 'Alice' situation in which you have to run very hard to keep in the same place. In order to realise what the wheat breeder is up against it will be useful to consider a list, necessarily incomplete, of objectives which





HYBRID VIGOUR GIVES INCREASED YIELD.—Fig. 1.—The two inbred strains of maize on either side were crossed to produce the vigorous hybrid in the centre.
Fig. 2.—Cobs of hybrid corn, showing remarkable uniformity.

Scientific plant breeding, such as that carried out at Svalov, has solved many farming problems in Sweden. New varieties have made the Swedes self-sufficient in wheat, and Prof. Harland mentions their new wheat/rye hybrid in his article. (*Photo from Svenk Filmindustri's film, "The Wizards of Svalov".*)

are being pursued in various parts of the world. These can be divided into five main groups:

GROUP I. *Resistance to disease*

The most important diseases against which resistance is needed are: stem rust, leaf rust, loose rust, smut, the take-all disease, mildew, foot-rot, fusarium.

GROUP II. *Resistance to insects*

Hessian fly, frit (Oscinella), &c.

GROUP III. *Physiological characters*

Resistance to sprouting after a wet harvest, to lodging, to grain softening, to cold (winter hardiness), and to drought. Tolerance to early sowing; capacity to respond well to artificial manures; earliness; long or short vegetative period; slow winter growth; medium tillering; late ripening; suitability for poor or salty soils; adaptability to combine-harvesting; high yield.

GROUP IV. *Morphological characters*

Non-shattering, optimum grain size, optimum number of grains per ear.

GROUP V. *Industrial characters*

Good milling and baking quality; high carotene content.

In spite of all the defects, some serious and others only trivial, of our modern varieties—and there is not one that the breeder would pass as perfect—many defects have already been corrected. Physiological and morphological bottle-necks preventing the attainment of full potential yield have been eliminated. The new stem rust-resistant varieties have produced in the USA an estimated annual increased yield of 41 million bushels valued at $27 million. Frankel (1947) tells us that in New Zealand the increased yield of a new variety, grown on less than 3000 acres, pays for the annual cost of all wheat-breeding in that country. In Sweden, new varieties have raised the yield of winter wheat by about 30%; high yielding capacity has been combined with winter hardiness, disease resistance, and stiff straw.

The best-known and most destructive diseases of wheat are the rusts, which are forever seeking a new ecological niche in which to spread. So we find that large numbers of biological races of rust exist, each of which has a specific power of infecting different varieties. To a single race of rust a given variety of wheat may be practically immune, manifest varying degrees of resistance, or be susceptible. But the rust may change to a more virulent form—a new race—and the whole wheat crop over a wide area may suffer catastrophic damage. So the pathologist who helps the breeder must keep an eye on all the races of rust and by means of observations often over a whole continent he must be able to say whether a new, virulent race has made its appearance and what modifications of the breeding programme are necessary to counteract it. The fight between the host and the parasite is never-ending, and some geneticists believe that some day there may occur a new race of rust which will attack all varieties, and against which the breeder may be powerless. Not long ago a new race of black stem rust made its appearance in Peru and crippled wheat-growing on the coast for many years.

The position with wheat may be summarised thus: most of the work of plant breeders consists in correcting morphological and physiological defects and in holding disease at bay. This type of work corresponds to the 'trouble-curing' section of industrial research. The yield of wheat is almost always limited by a complicated series of internal and external factors. A combination of poor and worn-out soils and drought can reduce a potential 80 bushel per acre variety to a 10 bushel per acre level. Yield is not solely the concern of the breeder. It is very much the concern of those who deal with the environment in which the plant is grown—the soil scientist, and the crop ecologist. We still do not know much about the way in which heredity and environment are integrated in the final yield. A growing crop is a large-scale experiment in applied plant

physiology. So we must now call upon the physiologist and the biochemist to join the present teams of geneticists, cytologists, plant pathologists, and statisticians.

Some workers hold that the yield of wheat in Great Britain either cannot be increased, or that further improvement would be so slight as not to be worth while. It seems likely, however, that an increase of the order of 20% could be obtained. The view that breeding for yield had reached its limit was widely held about maize before the growing of hybrid maize became widespread.

The weakest point about wheat as a crop compared, say, with maize, is the fact that it is mostly self-fertilised and thus is highly homogeneous. A good deal of the purity demanded by the industry is probably unnecessary. As Frankel says: "Purity is concerned with characters which are readily seen, but often are of little significance. Its excessive pursuit absorbs energies, delays progress and deludes the breeder and the farmer as to the real merits of crop varieties. I suspect that often it is little more than a commercial convenience."

As efficient biological machines, cross-fertilised plants such as maize or rye are better than self-fertilised plants such as wheat. The amount of genetical variability in a maize crop is so high that it doesn't, so to speak, pay a fungus disease to mutate to attack it widely. We therefore hear far less about epidemic disease in maize than in wheat. If it could be done, it would probably be a good thing in the long run to convert wheat into a cross-fertilised plant. But no existing research institution that I know of has the opportunity to do this sort of work. Quick economic results are demanded and most attention is therefore given to 'trouble curing'.

The synthesis of what are virtually new species has fascinating possibilities for the future. We have seen that it has happened in the past in the case of bread wheat, and we now know that the commercial cottons of the New World originated from hybrids of wild Asiatic and wild New World cottons. A new plant has been produced in Sweden by crossing wheat and rye and then doubling the chromosome number. This 'wheat-rye', now called Triticale, has all the characters of a new species, and if it can be further improved it may be an important addition to the world's food crops. In the USSR and the USA crosses of wheat have been made with some grasses such as *Agropyron* (couch grass) and *Aegilops*. Extravagant claims have been made in Russia for these new 'perennial wheats', but while this sort of work is one of the really important fields of the future, it has not yet made any major contribution to the world's food supply.

It is now clear that all sorts of wide crosses are possible in the grasses. Sugar-cane, for example, will cross with maize, bamboo, and sorghum.

But among all the possible methods of increasing yield, there is one which is invariably a colossal success wherever it can be adopted, and that is the utilisation of the hybrid vigour which results from the crossing of inbred strains or of commercial varieties of somewhat different origin. All our vegetatively reproduced crops such as potatoes, raspberries, strawberries, and many fruit trees have this hybrid vigour. That is one reason why seedlings from them are usually inferior to the parent variety. The plant in which hybrid vigour has been most used on a commercial scale is maize. Crosses between selected uniform inbred strains extracted from highly heterogeneous commercial varieties can give up to 25% increase in yield over the most productive of these commercial varieties. Hybrid maize has been developed most successfully in the USA and it is estimated that through its use about 700 million more bushels of maize per year are being produced. This is certainly the most brilliant and impressive practical contribution which genetical science has so far made to agriculture. Those of us who are in the game feel that we are just in the initial stages of the application of hybrid vigour to a large number of crops. We are all peddling our remedies for the desperate plight of mankind, but I think that we biologists could do a great deal more than we are allowed to do—given an environment in which creative work could flourish. But almost any biologist will tell you that he does not have such an environment and he is prepared to tell you why.

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PROGRESS OF SCIENCE—*continued from p. 308.*

in its forty-fifth year, indexes and summarises the world literature on pure and applied chemistry.

The applied journal, *Industrial and Engineering Chemistry*, is unique and read by industrial chemists throughout the world. The first issue of *Ind. Eng. Chem.*—as everyone calls it—appeared in 1909, and it included an article on Bakelite by its discoverer, L. H. Baekeland, one of the many distinguished chemists who have served as president of the A.C.S. First produced as a supplement on analytical chemistry to this journal, *Industrial and Engineering Chemistry*, *Analytical Chemistry* became a separate publication in 1948. Originally produced as a supplement to *Ind. Eng. Chem.* is *Chemical and Engineering News*, a weekly giving news of interest to members of the chemical profession.

Another useful A.C.S. publication is the *Journal of Chemical Education*, published by the society's Division of Chemical Education and devoted to the advancement of chemical education and professional training. Besides its periodicals, the society also publishes monographs of great value to the chemical profession.

The A.C.S. also maintains a news service, one of the first ever organised by a scientific association, to supply chemical news for the Press, radio and television. There is nothing comparable to the A.C.S. News Service in Britain, unfortunately.

Today the A.C.S., with its membership of 66,000, claims to be the largest professional body of scientists in the world. The membership is shared by 138 local sections—one of which operates in Hawaii, and yet another in Puerto Rico—and the society is organised to a large extent on a regional basis, and as one might expect it has some of the characters of a federation of regional societies.

La Caille: 10,000 Stars in Two Years

DAVID S. EVANS, M.A., Ph.D.*

In April, 1751, there arrived at Cape Town a young French astronomer, Nicolas Louis de la Caille, who had been sent to Africa with the blessing of the Academy of Sciences of France and the good wishes of his fellow savants to make certain astronomical observations. The colony was then ruled by the Dutch East India Company, and so powerful was La Caille's backing that labour for the construction of a small observatory in the yard of a house in Cape Town was rapidly forthcoming while, later on, when he was conducting survey operations, a white officer, slaves and transport were put at his disposal. This was the more remarkable since the local administration were still smarting from the recent behaviour of another astronomical visitor, Peter Kolbe, who had beguiled his time smoking, drinking, gossiping and smuggling derogatory reports about the officials back to Europe.

Thus La Caille's expedition never needed to struggle for recognition, and by the time of his return to Europe in 1754 he had become widely celebrated. Born near Rheims in 1713, he had received a classical education and acquired the formal title of Abbé. He appears never to have filled any religious office, and to have quickly turned to scientific pursuits, working first with the Cassinis at the Paris observatory and later as professor of mathematics at the Collège Mazarin where he set up an observatory and produced a vast mass of results. He had earlier been concerned with the measurement of the French arc of the meridian and published a variety of books on mathematics and astronomy, including a large table of eclipse predictions. He sailed for Africa in the autumn of 1750, visited Mauritius and Réunion in 1753, returned to France in 1754 and died in 1761. During the four years of absence he made the observations which have brought him permanent fame, and which entitle him to rank as the father of southern astronomy.

The variety of these researches as they are published in the *Mémoires* of the Academy makes the picture confusing, and the confusion is increased by the system of publication then adopted. A volume was issued for each year, usually several years in arrears, containing first a series of items of editorial comment on current scientific developments. This section is called the "Histoire". Then follow the separate original contributions, the "Mémoires". Owing to the delay in publication, the former items are usually far more up to date than the latter and often appear to be prophetic. Thus, for example, the volume for 1751 contains reports by La Caille up to 1753, and editorial comment up to 1755—the actual year of publication.

In order to appreciate La Caille's work, to understand his choice of research topics, and to appreciate the reasons why he was so strongly supported in official quarters, it is necessary to review the state of astronomy as it existed in the middle eighteenth century.

Modern science, usually reckoned as starting in the days of Newton, is then about a century old. By this time it is inconceivable that any prominent scientist could contemplate for a moment, as Newton did, the influence of astronomy on the judicial art of astrology. The outlook of the eighteenth century is in many ways indistinguishable from that of the twentieth. Superstition has gone. Fundamental problems of principle which rocked the scientific and theological worlds have been settled. The question whether the earth goes round the sun or vice versa is not at issue. The exact shape and dimensions of the earth's orbit are an immediate problem. It is well known that purely by observations of angles an exact scale model of the solar system can be constructed, and this has been done. In principle this method involves using the radius of the orbit of the earth as the standard of length and the determination of all other lengths in the solar system in terms of this unit. What still remains to be done is to determine the length of this unit in miles, a step which will, incidentally, involve measurements of the dimensions of the earth itself in miles. Dynamical astronomy has made great advances; eclipses can be predicted, so can the occurrence of eclipses of the satellites of Jupiter, and some progress has been made in the solution of the very complex problem of the motion of the moon.

The positions of the brighter stars in the northern sky have been fairly well determined and catalogued, but work in the southern sky lags far behind. In fact, Halley, on his expedition to St. Helena in the previous century, is one of the very few professional astronomers to have set eyes on the southern sky. The few constellations which have been described have been mainly the invention of the early Portuguese navigators.

This list of achievements may seem a miscellaneous catalogue of purely technical points of little interest outside professional circles. This impression is quite wrong. The astronomy of the day has most urgent and important practical applications to navigation. What is happening in astronomy is of immediate social and economic importance.

The period of long-distance trading voyages has fully begun. The ships of half a dozen nations range the seas from the spice islands of the Pacific to the Caribbean. Voyages are made lasting many months far out of sight of land. Even so the means of determination of position at sea are very inferior, and there have been numberless wrecks due to the uncertainties of navigation. The only known method of arriving at a particular place is to sail into the correct latitude—which is easily determined from the apparent height of the pole star in the northern hemisphere, or in a variety of other ways. Then, having made sure that the ship is, say, far west of her objective, to sail due east along the parallel until the destination is reached.

However, so uncertain were the means of longitude determination that a shipmaster could never be quite sure

** This is Dr. Evans's first article for "Discovery" since he joined the Royal Observatory, Cape Town—which was set up in the 1820's as sister observatory to Greenwich in the southern hemisphere. The Abbé de la Caille was the first astronomer working at the Cape to chart the southern skies.*

on which side of his destination he might be. When supplies were short or in enclosed waters the results might be tragic. Some years before, a whole squadron had been lost in thick weather through ignorance as to whether or not it had passed through the Straits of Gibraltar. On contemporary charts, the remoter islands would often be marked two or three times on the same parallel of latitude simply because nobody could tell what the true longitude might be, and estimates might vary by several hundred miles.

The Problem of Longitude

The principle of finding longitude is simple enough. From a vessel at sea the navigator observes the sky and notes certain features of what he sees, such as the altitude of selected bright stars. He derives his longitude by comparing, effectively, these results with similar results which would be obtained at the same moment by a second observer at a standard station on the earth's surface. If, for example, an observer at Greenwich sees a certain star due south, this establishes that at this moment the earth in its rotation is just carrying the Greenwich meridian past the star. If at the same time the navigator establishes that his meridian is passing a star 30 degrees west of the first star in the sky it will follow that the navigator's meridian is 30 degrees west of Greenwich. The method depends in principle on an instantaneous comparison of the appearance of the sky as seen by the navigator and as seen by the observer at the standard station. An obvious simplification will be to provide tables giving the results of typical observations which can be made at the standard station. This involves an exact knowledge of star positions and a capacity for predicting the positions of planets and other phenomena in advance. The modern Nautical Almanac is, in fact, such a compendium of all possible observations, computed in advance.

However, it is still necessary for the navigator to be able to fix the *time* at the standard station at the moment when he makes his observations. Today a good wrist watch, checked frequently against radio time signals, is quite adequate for navigation. Two centuries ago neither the wrist watch nor the time signals were available. Contemporary clocks were still of the pendulum type which, if disturbed, will systematically lose or gain beats. On the tiny ships of the day, which probably rolled like barrels, there was no hope of keeping a pendulum clock running to time. Several of the maritime powers had offered very valuable prizes for methods of determining longitude, and one of them was just about to be won by Harrison for his invention of the chronometer. So much unsuccessful effort had been expended on the problem that many experts believed it impossible to produce a sea-going chronometer, and had sought alternative methods of finding position at sea. Even when chronometers were later being introduced it was considered important to keep alive possible alternative methods as a check on errors of rate in the chronometer; to provide, in fact, an analogue to the regular check now provided by time signals.

Two alternative methods of position fixing were mooted. One sought some quantity which could be determined easily, which by its value would identify a particular place. The other sought a substitute for a clock of the ordinary kind. La Caille himself was interested in both these methods.

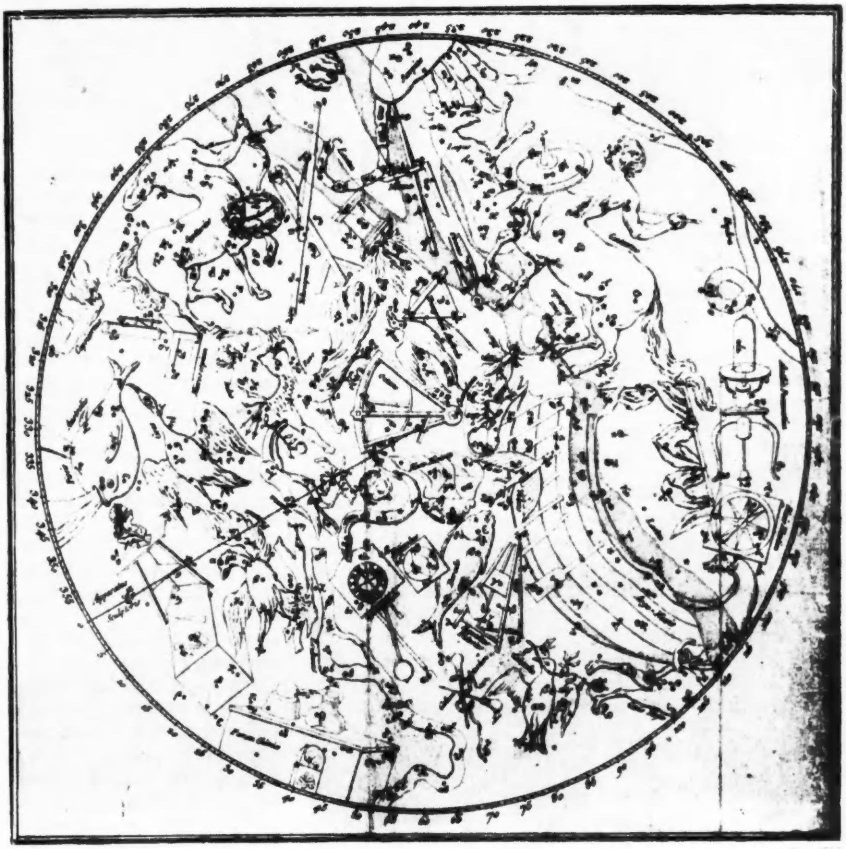
The first proposal was to identify places by their magnetic elements—the dip of the magnetic needle from the horizontal and its deviation (declination) from the north. An optimistic contemporary writer remarks that there will be no two places on the globe which will have, at one and the same time, the same latitude, the same dip, and the same magnetic declination. La Caille himself made experiments and found negligible magnetic errors due to the cannon on a 74-gun ship. In these proposals they appear to have been following the lead given by Halley in the previous century when he carried out an extensive magnetic survey. The investigators were quite well aware that the magnetic elements change with time, but merely regarded it as necessary to resurvey the magnetic elements every ten years. What brought the scheme to naught was probably the difficulty of exactly locating a position at sea in the first place, not the determination of the magnetic elements there.

Substitute clocks are provided by a variety of recurrent phenomena. Two favourites were the phenomena of the satellites of Jupiter and the position of the moon against the background of the stars.

The eclipses and occultations of the satellites of Jupiter could readily be predicted in advance. For an observer in a fixed position on shore they were ideal as a substitute universal clock. He used a pendulum to obtain his own local time by solar observations and then fixed the times of the satellite eclipses in this system. He at once arrived at the difference between his own local time and that at the standard station for which the satellite phenomena had been computed, and this gave the longitude immediately. It was by this method that La Caille determined the longitude of the Cape, of Rio de Janeiro on his voyage to Africa, and of Ascension on his return.

The motion of the moon against the background of the stars is not uniform but, given sufficient astronomical knowledge, is predictable. The moon serves as an irregularly moving hand passing over a clock face marked irregularly by the stars. On the average it moves about 13 degrees in angle per day, so that if the angular position of the moon is used to estimate the local time, any error in the measurement of this angle will be reflected as an error of determination of local time (longitude) which is 28 or 29 times as great. The method is therefore not accurate. La Caille estimated the errors of determination of position as 97 leagues (nearly 300 miles) and remarked that it was practicable only if somebody "fort exercé dans le calcul" was on board—a condition rarely fulfilled. In modern parlance this would run that the method could be operated if the Director of the Nautical Almanac Office were a passenger, but not otherwise. La Caille on his voyage away from the Cape to the isles of France and Bourbon (Mauritius and Réunion) worked out improvements in the method and in its application which, he said, brought the error down to 4 degrees and the method within the reach of any navigator.

When La Caille first sought the support of the Academy for his voyage to Africa he proposed three objects. One



THE ABBÉ DE LA CAILLE'S PLANISPHERE OF SOUTHERN CONSTELLATIONS

from *Coelum Australe Stelliferum*

was to chart all the stars in the southern sky down to the 3rd or 4th magnitude, an objective of obvious importance both for fundamental and navigational astronomy. He proposed to determine the longitude of the Cape, which was then the principal port of call for all vessels going to the East, and whose position was uncertain by several degrees. The longitude was later accurately determined by La Caille using the method of Jupiter's satellites already described.

The third objective was to make observations at the Cape for the determination of the parallaxes of the sun, moon and planets. All survey operations depend on the measurement of the angles at the base of a triangle, the base being of known length, the third vertex being the point whose distance is required. The same principle applies whether the base measures a few tens or hundreds of yards, the distant object being a point whose position is to be fixed on a large-scale map; or whether the base is several miles long, the third point being a beacon on the primary large-scale triangulation of a country; or whether the base line is a considerable fraction of the diameter of the earth, the distant point being one of the planets. It will be realised that in this last case the angle at the vertex of the triangle is very small and that what will be observed is a slight shift of position of the relatively nearby planet against the background of the stars. This is what is called a parallactic shift, and the parallactic shift corresponding to a displacement of the observer perpendicular to the line of sight through a distance equal to the radius of the earth is called the (horizontal) parallax of the body, and is a standard way of expressing its distance from the earth.

For observations of the parallax of the sun, moon and planets it is obviously desirable to make the base line as large as possible; and to choose the two stations on the same meridian of longitude. This second condition is desirable because then the two observers will both have the body to be observed, say the moon, at the greatest altitude and so best placed in the sky, at the same time. They will thus be observing essentially the same situation at the same moment and the only difference between them will be that they are making their observations from different points in space.

La Caille was well aware of these conditions. He chose the Cape for his southern station because it offered him civilised surroundings differing by nearly 90 degrees in latitude from Europe, thus making the base line long. The Cape was the only place which would qualify for consideration at that time, and, in addition, it had the great advantage of having the same longitude as Europe, Cape Town being on almost the same meridian as Berlin.

These advantages have, of course, remained and in the latest determination of the scale of the solar system made some years ago by the present Astronomer Royal the results from the European and Cape of Good Hope stations received the greatest weight.

Before leaving Europe La Caille set out his plan of work in a published document, the *Avis aux astronomes*, and as a result Lalande was sent to Berlin, while Bradley at Greenwich and Grischov in Russia carried out the observations which were required to combine with those of La Caille.

The results were afterwards reduced and although the values are all a trifle high they are of quite a respectable accuracy. For example the value found for the solar parallax was 10 seconds of arc instead of 8·8 seconds, making the sun-earth distance about 10% too small.

When La Caille arrived at the Cape he visited the Governor and was sent to lodge in one of the best houses in the town. His observatory was built in the yard, and consisted of no more than a small room measuring about 12 feet square, set with its diagonals on the north-south and east-west directions, erected on a heavy masonry foundation. In this room La Caille had two cruciform piers for carrying instruments, a pendulum clock and a bed. He had two sectors each of 6 feet radius, one of them carrying two telescopes, a smaller quadrant, and a variety of telescopes, one 14 feet long which he used for observing Jupiter's satellites.

Eventually La Caille far exceeded his planned programme of observations. Among his minor observations must be included: his determination of the length of the seconds pendulum, needed in the establishment of his own local time; his determination of the magnetic elements, a task which he carried out wherever he went, and which was probably inspired by its possible navigational application; his daily records of weather and tides, on which he based a summary of meteorological and observing conditions.

La Caille's map of the Cape Districts. His 8-mile base is just north of Groene Kloof. Hout Bay which he surveyed is on the Atlantic side of the Cape Peninsula (*Mémoires de l'Académie*).

His main work included the parallax observations to which sufficient reference has already been made, his catalogue of nearly 10,000 southern stars with a discussion on the refraction of the earth's atmosphere, and his survey work. The volume of research is astonishing, representing as it does two years' work by a single man; it is a mass of research which, on the astronomical side alone, might occupy quite a large observatory for a year.

For his catalogue La Caille used a telescope 28 inches long and ½ inch in diameter, an instrument which might be scorned by a sophisticated schoolboy today. This he mounted so as to observe through a small hole, thus effectively cutting off the blast of the south-easter which, so he remarked, blew steadily for two-fifths of the year and spoiled the seeing. In the field of this telescope he fitted a little brass cut-out in the shape of a rhombus. His mode of observation was to fix his telescope in a north-south direction and to note the stars as they drifted through the field. The mean time of appearance and disappearance at the edges of the rhombus gave the time of transit across the centre of the field, from which the star's right ascension (name of celestial position co-ordinate corresponding to longitude on the surface of the earth) could be deduced. The time interval between appearance and disappearance enabled him to determine how far north or south of the centre of the field the star passed and so gave the star's declination (celestial co-ordinate corresponding to latitude on the earth).

He divided the southern sky into a series of zones, 25 in number, each having a breadth equal to the field of his telescope, and pointed his instrument at each zone in turn. So assiduous was he that he made excellent progress and decided to extend his survey to the limiting magnitude which could be reached with his instrument, probably about magnitude 7. Eventually he observed nearly 10,000 stars, for which he himself later reduced the positions of about 1,900. His full catalogue was only reduced in the next century by Henderson, then H.M. Astronomer at the Cape, and published in 1847. La Caille's unreduced observations were published after his death, at Paris in 1763, under the title *Coelum Australe Stelliferum seu observationes ad construendum stellarum australium catalogum institutae, in Africa ad Caput Bonae-Spei.*

La Caille noted the blank spaces between the old northern constellations of Ptolemy's Almagest and the few invented by the Portuguese navigators. In the course of this proceeding he administered a resounding rebuke to Halley who, he said, had wished to curry favour with his sovereign by inserting a constellation, "Robur Carolinum", King Charles's Oak Tree. La Caille remarked in effect that it made a mess of the sky, and proceeded to draft his own constellations, which are those of the present time.

He scorned the fabulous creatures of antiquity and, like a forerunner of the Age of Reason, decided to name them after the instruments of the liberal and philosophical arts. He gave us, for example, 'Apparatus Sculptoris'—the sculptor's studio, now abbreviated to 'Sculptor'; 'Antlia Pneumatica'—the air pump; 'Fornax Chimica'—the chemical furnace—and so on. 'Reticulum' is his own little rhomboid reticule used for the star-position work. 'Mensa' is not any table, but 'Mons Mensa', the Table Mountain under whose shadow La Caille worked. He has arranged it so that the Larger Magellanic Cloud, which looks like a detached portion of the Milky Way, represents the 'table cloth', that mountain-top cloud which streams down the upper crags of the mountain in the blast of the south-easter ("ce vent furieux du sud-est") which so impressed La Caille.

It is a cause for a rather wistful smile that La Caille's 'modern' constellations seem to us, on his planisphere, hardly distinguishable in style from those of ancient times.

His remaining astronomical contribution was the construction of a table for the correction of apparent star positions due to the refraction of light rays in the atmosphere. It is a matter of considerable difficulty to determine these corrections for any one station but, by combining results from Paris and the Cape, La Caille was able to produce a double-entry table showing corrections for various values of the barometric pressure and temperature, which is completely modern in manner and execution.

It became clear to La Caille that he would have to wait at the Cape until the fair weather season of 1752–3 should enable him to get a passage on a ship. This gave him a period of several months after the completion of his astronomical work during which he gave his attention to surveying. He had already undertaken work of this kind in Europe, and in 1752 carried out a survey for the Dutch East India Company of the Hout Bay region, which now seems to have disappeared, possibly because of its military importance.

His *magnum opus* was the measurement of the arc of the meridian near Cape Town which he carried out with the assistance of a local military officer named Muller, lent to him by the Governor. The scientific background of the project was approximately as follows: Newton's researches had shown that the earth ought to be an oblate spheroid, the flattening being due to its axial rotation. If the radius of the earth at the poles were less than that at the equator, pairs of points on north-south lines separated by 1 degree of latitude ought to be closer together in miles in high latitudes than in low. In fact, measures of surface distances compared with the corresponding latitude changes should give the true figure of the earth. The Academy had already sponsored similar measures in France, Lapland and Peru. What was not clear was the question whether the southern half of the world had the same shape as the northern, or whether it had any regular shape at all. Astronomically the problem was involved with that of parallax measurement, for unless the true figure of the earth was known, the true base line (e.g. Berlin to Cape Town) used in the parallax determinations could not be found, and the results for the distances of the planets became uncertain.

One very curious proposal put forward was to invert all this: to find the parallaxes of the planets from various base lines and from the variation of the values to determine the deviations of the earth from spherical form. Fortunately this idea was never taken up.

THE ABBÉ DE LA CAILLE.
(*By permission H.M. Astronomer at the Cape.*)

It should now be clear how La Caille saw his survey problem as part of his general astronomical work. He began by measuring a base with wooden rods on what are now the Darling Flats, to the north of Cape Town across Table Bay. This base was 8 miles long and, using it as the basis of triangulation, he located a number of mountain peaks and his Cape Town observatory as points on the survey. He then determined the latitude difference between his most northerly station and his observatory, and compared it with the linear distance deduced from the survey. He was obviously much troubled to find that his results supported the hypothesis that the earth was a prolate, not an oblate, spheroid. This result he partly rechecked himself but could find no error, and it remained as a serious puzzle for some years. In 1820, Captain George Everest (as he was then), the famous surveyor, reinvestigated La Caille's work and in 1836 Sir Thomas Maclear, then H.M. Astronomer at the Cape, made a very thorough reinvestigation and extension. The site of La Caille's observatory was identified with some difficulty, the site of his base line not at all.

It appears that La Caille's error was due to the deviation of the plumb line at his southern station by the large mass of Table Mountain.

La Caille left the Cape on March 8, 1753, not, as he had hoped, for France, but in reluctant obedience to a Royal order, to visit Mauritius and Réunion. He did not reach France until the following year where he returned to the Collège Mazarin and his astronomical observations.

La Caille is, *par excellence*, the scientist who lived for science and nothing else. In none of the accounts does he ever appear as a definite personality; he has few friends and no emotions. He seems a man without a private life who appears to pour forth the flood of his researches and disappears into an obscurity in which those researches are at once the only light and the only memorial.

Human nature will automatically adjust itself to meet the changed conditions brought about by the impact of science and technology on civilisation. That thesis is one which only an optimistic Lysenkoist could believe in, yet in more obscurantist forms it lies behind many modern philosophies. This article propounds a different thesis, of particular interest as its author took an engineering degree at Durham University before he took Orders. He is now Professor of Moral and Pastoral Philosophy at Oxford. This contribution is the substance of a B.B.C. broadcast and we print it in response to numerous requests from readers who heard the radio talk.

Ethics and the Technical Revolution

PROF. V. A. DEMANT

There are two forms of adaptation necessary in life, and they are of opposite kinds. In one we have to adapt ourselves to new facts that cannot be altered or to situations we have wanted to bring about without realising all they imply. If a man gets married he must adapt himself to that relation and not behave as if he were still a dependent child. If you get a weak heart you can live long and well by cultivating a gentle rhythm of life.

Adaptation or Disaster

These are adaptations to desired or inevitable situations. But another kind of adaptation is called for when you discover that certain developments need counteracting because, though they may have done good in one way, they have unbalanced life as a whole. For example, serious thought requires a certain amount of solitude and quiet; but it is strange what queer and crazy ideas a man can get if he keeps away from others and never hammers out his thought with them. When he discovers this he will adapt himself by balancing his solitude with social intercourse. Here is another case: a community may go in for one main kind of product or crop only, because of quick gains in trade, and find that soon its economy is lop-sided and will cease to be profitable. Adaptation then requires a variety or rotation of products. Or, again, if you begin to suffer from some kinds of rheumatism and then adapt yourself by following the inclination to stop moving your joints, that will increase the danger of permanent immobility. The proper adaptation would be a kind of resistance.

The first kind we may call wise adaptation to change: the second a wise adaptation of change towards normality or health or the good life. It will be clear that if you practise the first kind of adaptation when the second is needed, you are heading for disaster. This is what I think Bertrand Russell advocated in his magnificent series of broadcasts called "Living in an Atomic Age".

He there set himself the noble task of clarifying some of the serious perplexities of our modern situation, and offered interpretations of it so that men may regain some direction of world affairs instead of feeling carried along helplessly in the wake of events. But he set about this task with a philosophy imbibed from an earlier period—I mean an earlier stage of those developments which have led us to the predicaments he is concerned with. He advocated a bigger dose of the same thing when really an antidote is required.

I do not think Bertrand Russell was doing this in all his judgments; but in his central assumptions, I would say he was. There were, indeed, great insights which show him to be not entirely a representative of the forces which make the technical age a danger. For instance, when he said that a successful life needs more than intellectual conviction, he was correcting an error which has informed much of recent history. Again, how right he was when he insisted that fear is not an effective motive for constructive measures of healing and for setting man's house in order. It inhibits instead of stimulating. But surely the motive he advanced in his second talk to take the place of fear and heroism, namely "a just estimate of self-interest", if not so numbing as fear, is, at any rate, not vital enough for great constructive purposes. Does anyone really believe that a just estimate of self-interest will solve our problems? Or that we can dispense with all heroism, the readiness to dedicate oneself to a cause at a cost—virtues which Lord Russell, running away with himself in an eloquent passage, attributes to an out-of-date ideal which admires the leader of a gang of pirates?

There was a third insight in Lord Russell's talks which formed a kind of major premise of his whole argument. It was that conflicts within man set him at loggerheads with other men. I have two observations to make here. While it is true that conflict often springs from disorder within men, it sometimes arises from a plain lack of something, as Bertrand Russell admits has been the case of wars in the past. And the conflict with nature, of which he had much to say, is necessary because nature will not give up her powers to man unless he wrestles to get them. It is an objective resistance he has to cope with. And Lord Russell tended to ignore the fact that conflicts between men or wars between peoples may come from objectively hostile behaviour on the part of others, which must be met by men or peoples who are not spurred to aggression by their own inner disharmonies. Here again I think he forced an argument from a true insight and gave it misleading applications because of his philosophy of human nature.

I now take up his main theme. To begin with, observe that it was couched in terms of a large number of 'ifs and buts'. If only men were more reasonable, co-operative, fearless and happy, then our worst dangers would be overcome. This is not a very effective type of persuasion, *because the hardest kind of problem human nature is faced with is not to find out what changes would be beneficial, but how to make them.* Many a drunkard knows he would be a better man if he were sober, and many a thief if he were honest, and a hot-tempered person if he were patient. To tell them these things is to offer what they know already,

and to have it said without the secret of power to change only adds to their inner conflict. I am a teacher of theology, and if a student for the ministry were to bring me an outline sermon for comment, I should send it back were it mainly a declaration of how much better the world would be if all men were saints. I would say this is not a sermon, a means of persuasion, it is merely a set of hypothetical statements which are so true that they help nothing.

Of course Lord Russell is not all the time saying we could have bacon and eggs if only we had bacon, provided we had some eggs. But, listening to his talks, I got something like that impression. In one important aspect he did indicate how he thinks we can acquire the virtues the atomic age needs, and indeed they turn out to be the old virtues of love, joy, peace, mutual help and courage. What he does want new is an estimate of our own nature which, he believes, will lead to a rooting out of ideas and dispositions inherited from the past. It is these ideas and dispositions which, in his view, make for fear, envy and conflict.

Effects of Specialisation

What are we to say to this argument which looks so logically convincing, even if we cannot see where to begin in this universal task of changing mankind? We should first, I think, examine Bertrand Russell's attitude to what he calls 'the atomic age', an attitude which he has not said much about. And because we do not yet know how much the use of nuclear energy is going to alter the pattern of our already highly industrial and commercial culture, I shall call it the 'technical' rather than the 'atomic' age. And we must be clear that when we speak of the technical age, we are referring to a combination of at least three things: its *power*, its *social structure* and its *philosophy*. There is, first, the use of machines and chemical processes; these by themselves are an undoubted benefit, and any dangers in the technical age do not reside in their use. I would say there is something theologically wrong in condemning machinery and applied science, for these are the products of the spirit of man using material to multiply the result of his efforts. But there is a second factor in the modern technical age—that is, a social and economic structure built up by the large-scale use of natural power—from coal, oil and now possibly energy released from the cosmic structure of matter, atomics. This second factor has made for increased specialisation between regions, extreme divisions of labour, conglomerations of human beings in towns to be nourished from other lands; it has altered man's age-long incentives to work, impersonalised human relationships, and—as Lord Russell was at pains to show—built up concentrations of economic power which are not easily changed in direction if the situation requires it.

And there is a third factor—namely, an idea or philosophy which gives a certain uncritical and incontinent character to the technical age. It is the view that if a task can be done more efficiently—by, say, technical means or by trade between economically specialised countries, or by centralised administration—therefore it will always, in the long run at least, be for the good of man, and ought to be done that way. This philosophy ignores that the raising of vast technical structures upon the more natural bases and associations of mankind is not just an addition to human powers; it can easily impair those powers by weakening the social and human roots from which they grow, and give a mechanical and impersonal bent to the human mind in the process. Many wise men are alarmed at the effects of what the French call the *déracination* of the human being in the modern world.

Wheat Gives Place to Factories

Now while Lord Russell is fully aware of one of these dangers, namely the menace to the reproductive power of the earth, which he dealt with in his powerful fourth lecture, he does not seem to think there is a problem in the effect the technical age has upon human beings and the roots from which they operate. He speaks as if the whole of this great development, with the three factors I have mentioned, were just putting a powerful instrument into men's hands, while the natural, associative and spiritual bases of life upon which the whole thing was reared remain unaffected. He therefore overestimates, for instance, the extent to which technical progress and large-scale production and exchange in trade indefinitely enrich even the economic basis of life. Only the other day there was a letter in *The Times* from a native of India which told how, when he revisited the United Provinces in 1949, he found that whereas in his childhood he had seen fields of corn and food grain glistening in the morning sun, he now saw vast stretches of sugar-cane—for export, I suppose—numerous factories and only occasional fields of wheat. That is only one instance to show that technical efficiency and commercial relations do not necessarily raise the level of livelihood.

But I am mainly concerned to question the idea that the social and economic effects of the technical age are things to which man should adapt himself, as if those effects were merely added ability to pursue his good aims and will surely strengthen the foundations of his life. It has for some centuries been assumed that such would be the result, and this assumption has been bound up with the philosophy which Bertrand Russell represents. The trouble began, as I see it, in the seventeenth century, when the ethical and religious tradition became separated from the scientific and the rational one. Responsibility no doubt lay on both sides—but the result was, on the one hand, that religion and ethics became an independent world of its own: and, on the other, the rational and scientific movements, making great strides in consequence of their independence, did not have to consider whether they were making for a world that really responded to man's total nature. In fact they took for granted that the moral and cultural aims they inherited were part of the very nature of man and would be with him under all conditions. Inevitably the spokesmen of the Age of Enlightenment could assume that the power now accruing to humanity would further its fuller life without question.

The easy conscience which Lord Russell bids us have was offered years ago by the creators of the new world, and widely accepted. The modern man on the whole has not been worrying about his sins for a long time—and along with this he has never thought it necessary to get a true

picture of total human needs, and has assumed that technical aids would always serve him well. And some of the conflicts in man which Lord Russell attributes to the sense of sin, and holds responsible for combative attitudes, are quite likely due to men not being really at home in the mechanical, impersonal world they have created.

Lord Russell says some strange things about the combative impulses. He finds, correctly I think, that they often spring from fear—but some of his reasoning perplexed me. Only victors in the age-long struggle, he said, have survived to give mankind its outlook. How, then, can fear be traced to the psychology of victors? And then he has told us that the struggles of the Hebrew tribes have given a combative bent to our minds through the influence of the Old Testament. But surely the Amazons and the Sea-Dyaks—some of the most martial peoples of the world—never heard of Joshua or David. But, leaving these small points, a more serious argument is that the sense of sin has poisoned our life and given us those inner conflicts that make us suspicious, aggressive and full of fear. I agree with him if he means a sense of guilt which the people who have it do not know how to expiate and relieve. Then, as many of us know not only from observation but from our own experience, a sense of guilt not faced and dealt with makes us stubborn, angry, more unjust: we keep on pushing our claims and wills in the same direction as caused the first pangs. By sheer persistence we will put ourselves in the right and beat down the claims of others. But sin is a definitely religious conception and is based on a relation to God. And a sense of sin brings a knowledge that the burden of guilt can be taken away by repentance and forgiveness. And when Christians refer to themselves as 'miserable sinners'—the word sinner is a label of *responsibility*. Man is not a weed or a rag, but a being who takes responsibility for himself. The word 'miserable' does not mean despicable, but needing the *misericordia* or mercy of God. It has been the sense that men are sinners in common as well as having the dignity of the image of God in them that has in the past led our civilisation to mitigate the extreme forms of conflict. There was a certain respect for opponents, devices of sanctuary, rules for preserving an enemy society's livelihood—as when the Greeks respected each other's olive groves—and later civilian slaughter was condemned. It was this sense of a common sinfulness that prevented the conviction of being relatively in the right from meaning that oneself or one's side was so absolutely good that any means could be justified to crush the other.

These influences which civilised conflict—though we may not think much of the total effect—were real, and they have been steadily swept away since the eighteenth century—when men acquired the easy conscience Lord Russell wants us all to have more thoroughly. Would he, I wonder, contend that "the long agony of remorse" of Orestes and Electra in Euripides' play *Electra* was a set-back from the questionless ruthless fulfilment of the blood-feud in the corresponding drama of Sophocles—where the heroes certainly have no sense of sin. Or did the spirit of Bolingbroke doing penance for the murder of Richard the Second stand for a humanising of political struggle or did it, as Lord Russell suggests, aggravate the combative instincts? I feel sure that he is glad, in the appropriately subdued kind of way, as I certainly am, that there is a sense of guilt for the use of the atom bomb—and wish it were more widespread. Undoubtedly a sense of guilt for something not repented of, may lead to panicky aggressive courses—but that risk is better than killing the moral scruples.

But this question of sin and guilt is not the central point, though Lord Russell made it the root cause of human disharmonies. It comes close to the main question, however, in this way. The period of increasing technical and economic development was one marked by a decline in the sense of sin and also by a decline in the belief that man had a definite nature, and that changes were to be scrutinised as to whether they corresponded to that nature and its perennial needs. And so I think there is a connexion between loss of the sense of sin, and loss of control over events.

The Precarious Crust of Civilisation

Lord Russell, as we should expect, has seen so many real issues that confront us of which we should not be aware if we only read the newspapers. But there are one or two matters on which he seems to have learnt nothing in the course of his long and illustrious life. He is still a child of the eighteenth century in believing that reason overcomes passion, whereas men need a religious discipline to give reason vitality and passion rationality. And he is also a child of the nineteenth century in misapplying the doctrine of biological evolution to the social history of mankind. So he regards all the forces which militate against the perfecting of human existence as a hangover from the past, which has to be outgrown. His great Cambridge contemporary, John Maynard Keynes, had the same outlook in his younger days. He records in one of his *Two Memoirs*, "We repudiated all versions of the doctrine of original sin, of there being insane and irrational springs of wickedness in most men. We were not aware that civilisation was a thin and precarious crust erected by the personality and the will of a very few and only maintained by rules and conventions skilfully put across and guilefully preserved. We had no respect for traditional wisdom or the restraints of custom. . . . It did not occur to us to respect the extraordinary accomplishments of our predecessors in the ordering of life (as it now seems to me to have been) or the elaborate framework which they had devised to protect this order. . . . As cause and consequence of our general state of mind we completely misunderstood human nature, including our own."

The problem raised by Bertrand Russell is the dilemma that a highly technical age with closely knit interdependence makes the forces of egoism much more liable to cause universal calamity than in earlier periods. His solution is the Utopian task *of completely changing human nature.* You might expect a moralist to endorse that wholeheartedly. But I do not, and I regard no programme as a moral solution which demands a universal change that cannot begin somewhere in particular—and here where we are. The seriousness of our situation is due to a combination of the character of our civilisation with the imperfection of man. This imperfection has been with

man always, and modern man has in no wise an extra dose of original sin in him. In fact there is among western peoples a large sense of our common humanity. People will give generously to a famine-stricken area; our dangers lie in the structure of modern life, which as in the case of India quoted, has made famine by a blind following of an alleged technical and economic advance. And there are other ways in which this uncontrolled drift made for strains and stresses which render human imperfection very disruptive. The close interdependence of over-specialised economic areas is a cause of trouble rather than of harmony. With the spread of the industrial arts, more people are competing for the food and raw materials from dwindling sources of these things—and competing by sending away more and more machine products for each mouthful of food. There is also the urge for technical pre-eminence in many peoples not at all suited for it—with a consequent inferiority feeling (quite unfounded) that makes them aggressive.

Again, the weakening of all natural communities, of local loyalties and of spontaneous associations—the bonds of craft, kinship, profession and creed—leaves men with little else but economic bonds and the sole legal association of the state. It is this empty social space between the naked individual and the naked state that tends to canalise all community impulses into nationalisms of a rivalrous kind. The disruption of the smaller areas of community by the extension of vast impersonal relationships and the lack of satisfaction in the work life of masses of people breeds the kind of inner disharmonies which Lord Russell sees to be at the root of unco-operative behaviour. Moreover, loss of community feeling and loss of a sense of significance in work tend to throw people back upon self-interest and economic gain as the sole motives for doing anything. And loss of true social motives cannot be compensated for by an increase of leisure, for the same uprooted mentality prevents the leisure from being a healing and recuperative and co-operative thing.

I have tried to answer the main question with an account very different from that of Bertrand Russell. Our varying answers both require changes which are not easy to make. He hopes for a change in mankind everywhere, which I think is a depressing view, for if you cannot begin anywhere without a change all over, you will in practice produce the feeling that nothing can be done. My view leads to the need for abating the uncontrolled drift of the technical age and giving it some sort of order so that the benefits of machinery and applied science become an aid to communities sound in their personal, community and cultural life, where the technical drift is now undermining them.

This you will say is a programme just as impracticable as the other. It seems impracticable because it is against the grain of the current philosophy of our age, and runs counter to the direction in which the modern states are trying to cope with their difficulties. And men who are valiantly trying to deal with their problem in a certain way, and have built up complicated equipment for it, find it hard to discover and admit that the problem is of a different kind. Of course, in order to counteract the defects of the technical age—that is to say, in order to make life more balanced between the technical elements and the sources of community life—there will have to be great readjustments: changes, for instance, in the way many people get their living, a wide redistribution of social power so that it is not concentrated in any one group, and, above all, changes in outlook. Such changes need not be like the uprooting of a plant, but training it to grow in a different direction. The vital thing is for leadership to see the necessity and begin to find ways and means. With the more balanced society which I am sure is needed, there will come the beginning of international co-operation instead of international entanglement. Each community will be able to look at others without depending too much for its own excellence upon their behaviour. There can then be a true international concourse of thought, science, art, travel and friendship and a mutual exchange of surplus products. International connexions will bring enrichments to countries sound in themselves, where such connexions are now inexorable necessities for survival and power. For man cannot truly love his neighbour unless he has some strength to spare which comes from his being a complete man, and this applies to societies as well as to individuals.

THE CLOUD CHAMBER

C. T. R. Wilson's broadcast in a recent Science Survey was, though it is doubtful if many people realised it, a historic event. It was so modestly done that any listener unacquainted with the physics of the past half-century could be forgiven for not savouring the occasion fully. Yet Dr. Wilson, now in his eighty-third year, is the inventor of a device so deceptively simple, so adaptable, and so universally used that it could almost be said that there would have been no atomic physics without it. There is a little exaggeration in this statement; it is more exact to say that not one atomic or nuclear physicist of the past forty years has worked without the help of the Wilson device.

This apparatus is called the *cloud chamber*. By means of it the tracks of electrical particles and rays, including beta-rays, electrons, alpha-particles, protons and X-rays—but *not* neutrons—are rendered visible. The length and shape and nature of cloud-chamber tracks gives information of the most refinedly quantitative sort about the particles that cause them. The evidence for the first atomic transmutations found by Rutherford was the photograph of such a track.

The evidence the cloud chamber makes available is circumstantial, for nobody can see a proton or an electron. Yet the evidence of the cloud-chamber track is as convincing as the vapour trails of invisible aircraft high up in the blue sky. From such trails much can be inferred about the behaviour of the aircraft even when these are themselves invisible, and the same is true of the vapour trail of an electrically charged particle—which is what a cloud-chamber track is.

So wide is the use of the Wilson cloud chamber that its operation is now recognised as a separate technique. This

is emphasised by the appearance of a new book, the first of its kind we believe, which is devoted entirely to the cloud chamber. It is by Dr. J. G. Wilson (no relation of C. T. R. Wilson), who is senior lecturer in physics in Manchester University. (Incidentally it was at Manchester that C. T. R. Wilson took his first degree.) The title of the book is *The Principles of Cloud-Chamber Technique* (Cambridge University Press, 1951, 127 pp. and bibliography, 15*s*.). It is a book for post-graduate students, and will be no use to the beginner or layman. It might perhaps be criticised as being bare of interesting facts associated with the history of the cloud chamber, but to offer such a criticism would be to carp at the author for not doing something he never intended to do. In a way the book is a memorial to C. T. R. Wilson, almost a birthday present in fact for it was published just twelve days before his eighty-second birthday.

Readers will be interested by the historical background of this instrument, hence the following brief survey of the way in which this apparatus has been developed from its simple beginnings over fifty years ago.

The story starts in 1895 when Mr. C. T. R. Wilson was a Clerk Maxwell student at Cambridge. He was dissatisfied with the existing knowledge about supersaturation in a gas. This phenomenon needs a word or so of explanation. A gas is a mass of molecules which are separated by relatively huge distances, so there is room for molecules of a second gas to go into the spaces between the molecules of the first gas. The most familiar instance is that of air containing molecules of water vapour. The greatest amount of water vapour that can be accommodated in the air depends only on the temperature—this is one of the well-known gas laws. When a gas has as much water vapour as it will hold it is said to be *saturated.* Now if this saturated gas is cooled, the maximum amount of water vapour that it can hold is diminished. So the excess is deposited as water in the form of rain or dew or mist, and this is what happens when clouds form in our atmosphere. But under certain circumstances it is possible to cool the saturated gas—this can be done by expanding it suddenly, for example—without any water condensing. In this state the gas is said to be *supersaturated.*

Several people, Helmholtz among them, tried to investigate this condition; some attained the condition by forcing steam into air. But in 1895 Wilson was excited by the fact that he found the existing evidence about supersaturation to be thoroughly unsatisfactory. He considered that none of the experimenters had secured the right conditions, and so he set out to devise an apparatus more suitable to careful and exact investigation.

He gave some account of his first cloud chamber to the Cambridge Philosophical Society, and then in 1897 contributed his first paper on it to the *Philosophical Transactions of the Royal Society.* (This latter paper was communicated to the Royal Society by J. J. Thomson, who was Wilson's chief and in whose patient struggles with vacuum pumps the problem of supersaturation had cropped up and perhaps suggested itself as one suitable for a research student to tackle.) In Wilson's paper the second paragraph was:

> *What is the limit, if such exists, to the degree of supersaturation which can be attained without condensation taking place throughout the moist air, is a question of considerable meteorological as well as purely physical interest. It was primarily with the object of finding an answer to this question that the experiments to be described were undertaken, such experimental evidence as already existed on the subject being of a very incomplete and contradictory character.*

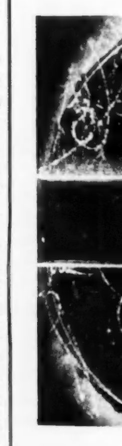
This may not seem at first sight a very exciting problem. Yet in the years that followed the researches that grew out of this unexciting paragraph were to lead C. T. R. Wilson to Fellowship of the Royal Society, a professorship at Cambridge, a Nobel Prize in 1927, and the Companionship of Honour in 1937.

That particles of dust and other substances acted as nuclei and facilitated the deposition of water was well known—this fact has been exploited in recent techniques for producing artificial rain. But any such nuclei, which C. T. R. Wilson lumped together and called 'dust', could be eliminated either by filtering the gas or else by expanding it suddenly and allowing the condensed drops to settle. C. T. R. Wilson was not concerned with this sort of condensation: he was interested in the fundamental properties of the supersaturated gas when it was free of all such nuclei.

His first apparatus consisted essentially of a sort of diving bell. It was a glass vessel upside down inside a large vessel containing water. The air was imprisoned inside the inner vessel. By releasing air above the outer water into an evacuated vessel adjoining, the outer water rose and so flowed out of the diving bell—he used the expression *cloud chamber* even then, in 1897—and so the air expanded. This was done so suddenly as to seem instantaneous. This was essential, the expansion had to be *sudden*; it had to be, to use the jargon of physics, an *adiabatic expansion.* If this condition was not obtained, that is if the expansion was *slow*, the gas had time to absorb heat from the surroundings, thus preventing the essential cooling condition required. Wilson gave arguments to show that it was just in this failure to get a sufficiently adiabatic expansion that Helmholtz and others failed.

First of all Wilson expanded and contracted the gas several times to get rid of the 'dust'. Then he found that he could expand the gas up to a volume 1·25 times its initial volume without producing any condensation. But at about that degree of expansion, drops were formed in small numbers and soon fell to the base of the cloud chamber. He was able to increase the expansion to 1·37 times the initial volume without producing anything other than this meagre rain. But at a certain volume after that, with an increase as small as that from 1·37 to 1·38, a multitude of fine drops was produced, and they fell very slowly. He called the conditions the 'rain-producing' and the 'cloud-producing'. (When the expansion was increased still further, a remarkable series of vivid colour changes was produced, but this is irrelevant to the present discussion.) By varying the experiment he showed that neither the conditions of illumination nor any filtering affected his results at all. Always there was rain at an expansion ration of 1·252 and a cloud between 1·37 and 1·39 and upwards. These were his first results. They came from hundreds of experiments performed in the summer of 1895.

His next set of experiments was on the effect of X-rays on the cloud chamber. It was known that X-rays made air electrically conductive, and, of course, everyone in 1897 had to do experiments with X-rays, for they were the



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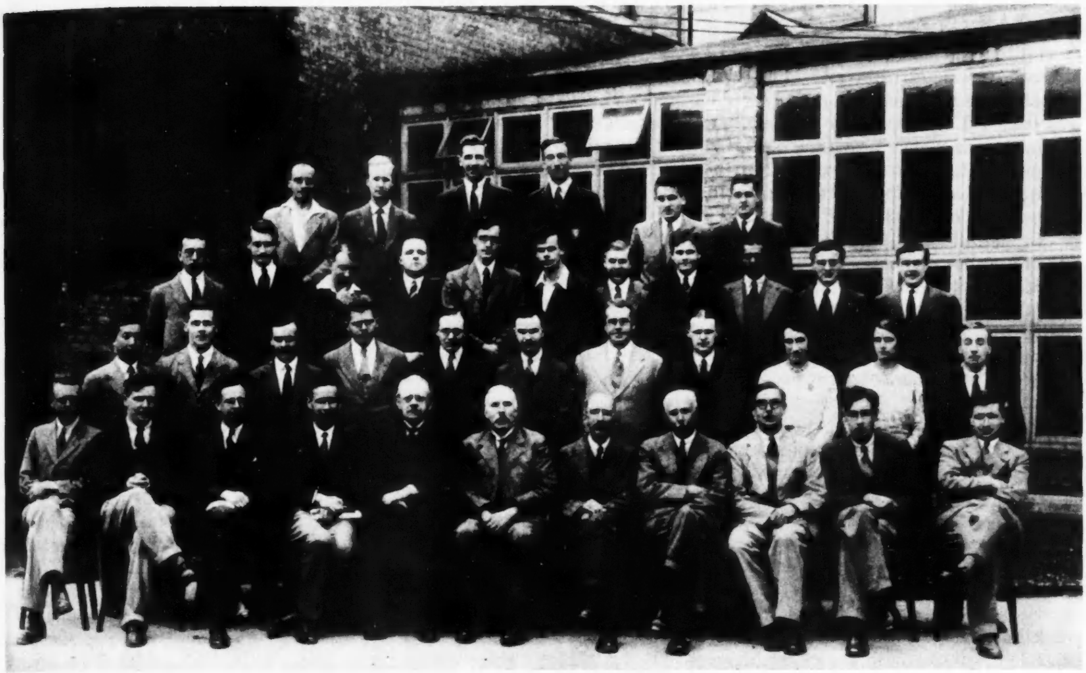
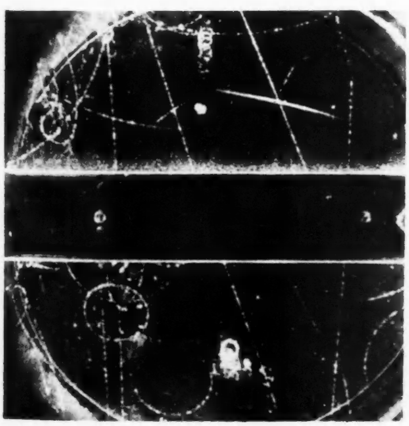


In this photograph of Cavendish research students taken in June 1932 Prof. C. T. R. Wilson is seen sitting on Lord Rutherford's right. The key to the whole group is as follows:

Back Row: N. S. Alexander, P. Wright, A. G. Hill, J. L. Pawsey, G. Occhialini, H. Miller. *Second Row*: W. E. Duncanson, E. C. Childs, T. G. P. Tarrant, J. McDougall, R. C. Evans, E. S. Shire, E. L. C. White, F. H. Nicoll, R. M. Chaudhri, B. V. Bowden, W. B. Lewis. *Third Row*: P. C. Ho, C. B. Mohr, H. W. S. Massey, M. L. Oliphant, E. T. S. Walton, C. E. Wynn-Williams, J. K. Roberts, N. Feather, Miss Davies, Miss Sparshott, J. P. Gott. *Front Row*: J. A. Ratcliffe, P. Kapitza, J. Chadwick, R. Ladenberg, Prof. Sir J. J. Thomson, Prof. Lord Rutherford, Prof. C. T. R. Wilson, F. W. Aston, C. D. Ellis, P. M. S. Blackett, J. D. Cockcroft.



(*Right*)—The classic cloud-chamber photograph, obtained by P. M. S. Blackett in 1925, showing artificial transmutation of an element. An α-particle (its track is in the centre and almost vertical) has hit the nucleus of a nitrogen atom, yielding an oxygen nucleus with the emission of a proton which flies off to the right.

(*Above*)—Cloud-chamber tracks produced by cosmic rays. (*From "Cosmic Radiation", Butterworths Scientific Publications, 1949.*)

newest thing. He then went on to use 'Uranium rays' (i.e. alpha-particles) and ultraviolet light. He found a new fact. It was that if the expansion-ratio was more than 1·252 but less than 1·37, or, in other words, if the supersaturated condition was that in which the meagre drops were normally produced, then irradiation by X-rays produced *clouds*. He found that the alpha-particles as well as those actually produced by these rays were of the same nature and were, he supposed, ions. That is to say, they were of molecular dimensions—he calculated a rough figure for the size of the drops produced—and had an electric charge. He showed the existence of the electric charge by applying a battery across from roof to floor of the cloud chamber, whereupon the ions disappeared quickly.

On the whole, these conclusions were the only ones that came from his experiments up to 1899. He had answered the question posed in 1895. After this he set out to improve the apparatus and make the cloud chamber suitable for photography and get rid of certain distortions seen in his early cloud chambers, which were more or less spherical. And then came a new emphasis. Up to 1900 and perhaps later he had been interested only in the physical and meteorological aspects of supersaturation. But in the early years of this century Rutherford and others were at work on radioactivity, and the actions of alpha-particles, beta-particles, and gamma rays were exciting most of the advanced physicists. So at some time between 1899 and 1911 C. T. R. Wilson began to think of his cloud chamber as a possible new tool in the investigations of these particles and rays. He could utilise the production of fog by the particles in order to show up their tracks with greater definition if he refined his apparatus and made sure that only the ions produced just at the time of the expansion and of the irradiation should be the ones photographed. He achieved this by keeping an electric potential between the roof and floor of the cloud chamber and by making an ingenious arrangement of spark gaps to fire off the illumination at just the moment after expansion. In this way any ions produced before illumination were rapidly disposed of by the electric potential and only those created in the fraction of a second of the illumination were photographed.

His success was remarkable. He obtained clearly defined white tracks against a black background. One track of an alpha-particle, showing one slight bend and one very sharp one, interested him very much. He had this photograph with him one day when he happened to meet Sir William Bragg. He excitedly produced this photograph to show the professor, who was astonished, for the shape of the track was almost identical with what Sir William had forecast on theoretical grounds and had not yet published. This corroboration of the success of the technique was very encouraging. His first paper about it was published in the Royal Society Proceedings in 1911: "On a Method of Making Visible the Paths of Ionising Particles through a Gas". This was followed in 1912 by his second paper: "On an Expansion Apparatus for making Visible the Tracks of Ionising Particles in Gases and some Results obtained by its Use". In these two papers he laid down the principles of the cloud-chamber technique for the investigation of fundamental particles.

His cloud chamber by this time was a shallow cylinder of glass, some six inches or so in diameter, on a floor of brass which was itself a plunger. This plunger could be made to fall very suddenly by triggering its connexion to an evacuated globe. At the same time the triggering made a ball fall from a counterweight and in falling pass between two balls joined to Leyden jars charged from a Wimshurst machine. (This may have historical significance. It may have been the last time that a Wimshurst machine was used for anything other than a schoolroom demonstration. Is the dusty Wimshurst machine to be seen in the dark recess of a landing in the Old Cavendish Laboratory the one actually used by C. T. R. Wilson?) The high voltage of the spark produced when the falling ball went between the connexion to the Leyden jars operated a home-made mercury arc, which illuminated the cloud chamber for the few moments necessary for a photograph to be taken. The inside of the cloud chamber was lined with gelatine to prevent the fogging of the glass window, that on the floor being blackened with Indian ink. Parts of this apparatus are still preserved in the Cavendish Laboratory.

The war of 1914–18 prevented further work and it was not until 1923 that two more papers appeared. Essentially the apparatus had not changed, though improvements had been made. The gelatine was gone. The falling weight had been replaced by an ingenious arrangement of pendulums, which could be adjusted by means of sliding weights. The photography was stereoscopic. Some 500 pairs of photographs were taken by C. T. R. Wilson for these papers, one on X-rays and another on beta-particles. It was more or less the standard design of cloud chamber for everyone to copy and soon the instrument-makers were producing cloud chambers for sale. There was even a hand-operated demonstration model with a small spot of radioactive material at one side. To operate this one watched the cloud chamber and operated the expansion in the hope that a radioactive particle would shoot across at the right moment. If it did, its track was clearly seen.

In 1933 C. T. R. Wilson, by this time the winner of the Nobel Prize, made another improvement. One disadvantage of the older cloud chamber was its comparative complexity in that every part had to be most accurately machined and fitted so that the operation should be efficient. Another disadvantage was that the chamber had always to be used upright so that gravity would draw the plunger down on the removal of the gas pressure underneath it. So in the new design there was no plunger. Instead the floor of the chamber was a perforated gauze and the expansion was obtained by first compressing the gas and then releasing it by the usual technique of connecting it to an evacuated vessel. The two sorts, the old and the new, are described by J. G. Wilson in the book mentioned as 'volume-defined' and 'pressure-defined' respectively. The new type could be used in any position and so was suitable for many cosmic-ray experiments.

The rest of the story is highly technical and belongs to J. G. Wilson. In his book he goes into mathematics of design and use, and there are graphs and diagrams in plenty. But there is not one cloud-chamber photograph. This may be significant, for it may mean that what was once an interesting and novel technique has become now such a normal part of the atomic physicist's routine that a photograph is not even considered necessary.

C. L. BOLTZ.

British Association at Edinburgh

This year's meeting of the British Association for the Advancement of Science—its 113th annual meeting—was extremely well attended. Its membership of 4012 broke all records, with the exception of the attendance at the centenary meeting in London in 1931. It far exceeded the average membership for Scottish meetings of the Association, which before this year's meeting was less than 3000.

The colourful inaugural ceremony was held in McEwan Hall on the evening of August 8, when the Duke of Edinburgh received the honorary degree of Doctor of Laws from the Chancellor of Edinburgh University, the Marquess of Linlithgow, before delivering his presidential address. The Duke's speech, which was reproduced in full in the last issue of DISCOVERY, was remarkably successful, striking as it did a happy balance between factual content and well-informed opinion. Its success did in fact suggest that British Association presidents ought to try to emulate the Duke in one respect: The Duke had paid as much attention to the presentation of his factual material as to the selection of facts, with the result that his speech was a pleasure to listen to, and not just an address requiring concentrated reading in print.

MANY of the sectional presidential addresses were historical surveys of progress in various fields, and by their very nature do not lend themselves either to summarising or abstracting. For example, Sir David Brunt's address to Section A was a succinct survey of the past century in meteorology and calculated to appeal mainly to professional meteorologists. Only a few points he made held any general interest. There was, for instance, this pregnant remark about the issue of the first weather forecasts and weather maps to the newspapers in 1861: "The use of weather maps for such purposes was regarded in scientific circles with suspicion, and was described as 'empirical', a word which, in the mouths of scientific men, is a heavy missile." (The position has changed a great deal in the intervening period; today only the layman apparently is sceptical about these things!) The official met. service in Britain had taken shape by 1920, the only organisational change since that year being the removal of Naval Meteorology back to the Admiralty. The two world wars have accelerated advance in meteorology; the recent war stimulated instrumentation in this field through the application of radar and short-wave radio to meteorological problems. Observation from high-flying aircraft has revealed a new phenomenon—the *jet stream*. This very fast stream of air occurs at levels of 15,000 feet–40,000 feet above sea-level, moves at speeds of 130 knots or more, in a general west-to-east direction, and can be traced for great distances downwind, though it is sharply restricted in the vertical and the transverse directions. On the poleward side the decrease of speed attains as much as 100 knots in 100 miles, while on the equatorial side a decrease of 100 knots is only attained in 300 miles from the central axis. In the vertical direction the decrease of horizontal speed may attain 80 knots in a range of height of 12,000 feet upward or downward from the axis of the stream. Thus the jet stream is a relatively narrow, intense stream of fast-moving air, the possible occurrence of which must be taken into consideration in plans for flight in the stratosphere.

Sir David deplored the way Britain lags behind in the teaching of meteorology; the chair of meteorology at the Imperial College of Science and Technology (which Sir David occupies) is as yet the only one in the whole of the Commonwealth.

Science's Credit and Debit

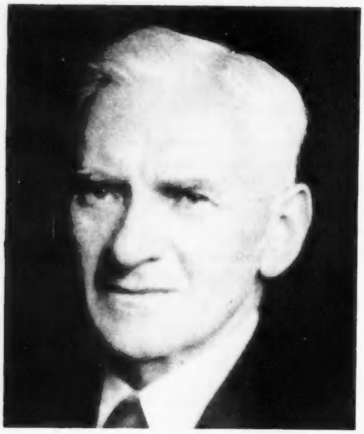
One challenging and provocative address by a sectional president was Sir Cyril Hinshelwood's to Section B (Chemistry). Early in his speech he delivered a spirited defence of scientists in general and chemists in particular against the charge that they take insufficient thought about the way their services are used by the community, services which can be harnessed to the purposes of war with terrifying results. He said that the same virtuosity which the chemist brings to the synthesis of dyes surpassing Nature in variety and often transcending it in beauty, of drugs which heal deadly sicknesses, polymers which adapt themselves to every diverse need also produces new explosives of increased power and poisons of greater potency. "But", said Sir Cyril, "can it seriously be laid at the door of the studious minority if the agents they produce to blast rocks are prostituted by the majority to blast one another? And in any case, though the precise calculation would be difficult, I should be prepared to venture the assertion that in the past century the good wrought by drugs, antiseptics and anaesthetics in saving lives and alleviating suffering far outweighs the evil which explosives and poison gases caused in wars. And speaking of the future, it is at least a possibility that the control of cancer may emerge from the detailed chemical study of cell mechanisms.

"Increasingly efficient machines, they are fond of telling us, are used for dropping destruction from the skies, but there is no scientific reason for this pastime. Nor have the works of man ever vied in destructive power with the vagaries of the Yellow River or the dire invasions of the great plagues. The beneficial uses of energy, analysed in Sir Harold Hartley's masterly address of last year, far outweigh the destructive. At the most primitive level they save us from death by freezing, and in more subtle ways the complex special fuels which the chemist now brings forth prepare the way for a unification of the world which is only impeded by vices of a kind having no connexion whatever with science.

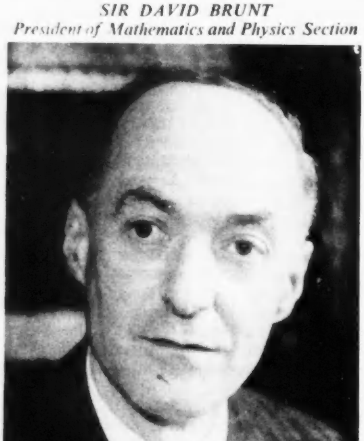
"What is the power of destructive weapons compared with that of lying propaganda? And was this product of the the devil begotten by a man of science? The inventions of applied science may attack men's bodies; those of seemingly humaner agencies can destroy their souls. To wish to inhibit or restrict scientific discovery is to show an utter lack of faith in human destiny. If there is any controversy about this, then it is not men of science who are blind to higher values, and in any case it would be preferable merely to be blind to higher values than to use an arrogant personal conception of them to stifle the pursuit of truth by others."

Sir Cyril spoke of the chemical basis of the living cell; "the wonderful skein of reactions out of which the life process is woven" was the phrase he used. He described the growth of a cell as involving an elaborate symphony of chemical reactions, the rules of which are slowly and surely being discovered. "What is dimly appearing includes the mutually aided autosynthesis of protein and nucleic acid, changes in proportions of enzymatic material in response to the chain reaction velocities imposed by various environments, and discontinuous modifications in the molecular pattern caused by radiations or the accidents of abnormal cell division. The cell with the relatively stable structure of the molecular patterns which are the basis of its genes is a system of great traditional conservatism, but one which exhibits also response to change both of a long-range and of a short-range character, change imposed both by chance and by environment. Over and above this, we see the mass-mixing of chemical characters when cells undergo processes of sexual union."

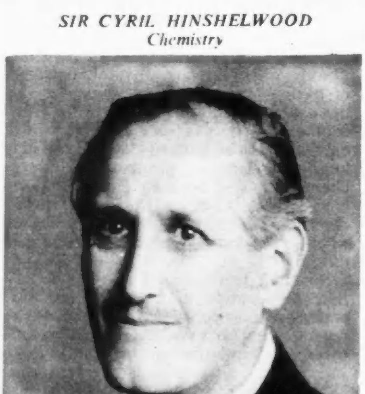
Sir Cyril Hinshelwood went on to say that the selective influencing of cell processes occurs in many ways, and opens the door to the great practical field of chemotherapy. Chemotherapy itself is still in its infancy, and generations may well pass before it fulfils all its promise. There is ground neither for facile optimism nor for gloom, but simply a challenge to patient resolution. Growth must be long and difficult, because as yet the rational chemical basis for much that we know is incomplete. But the first dim outlines of it are certainly there.



SIR DAVID BRUNT
President of Mathematics and Physics Section



SIR CYRIL HINSHELWOOD
Chemistry



PROF. W. B. R. KING
Geology

But even these things have another, more sinister aspect, said Sir Cyril. "As the cell reactions disclose their secrets, as physiology advances, and as the relation of chemical structure to effect on cell and tissue clarifies itself, there will emerge the possibility of deep-seated chemical intervention into processes which are now normally inviolate. Chemically induced mutations of cells are already known in a crude fashion, the influence of drugs on personality already exercises medicine and law, and the day may well come when a conscious moulding of individuals and even of races will present problems of fearful fascination.

"*If this day does indeed dawn the sky will ring more wildly than ever with cries against science, and the old battle of ultimate values will be joined more vigorously than ever. But it will still be those of little faith who fear the conscious intervention of mankind in the fashioning of its own destiny and who oppose what could equally well be represented by those so minded as part of a great purpose.*"

Sir Cyril concluded his speech with these words: "If in face of all these hopes and fears, doubts and assurances, we ask for a policy, there is no principle more pertinent, more sane or more necessary than that of Voltaire's hero, Candide, 'Il faut cultiver notre jardin'. If the chemist follows this homely advice and unconcernedly cultivates his own, it will not fail to go on producing its flowers and fruits in undiminished profusion."

Military Geologists

Professor W. B. R. KING's address to Section C (Geology) was entitled "The Influence of Geology on Military Operations in N.W. Europe". This speech will not stand condensation, and readers interested in this topic should consult the full version in the September 1951 issue of *The Advancement of Science*, the quarterly published by the British Association. The contribution of geologists in the British war effort has been well described in DISCOVERY (1946) by Professor W. D. EVANS, but one quotation from Professor King's address is justifiable, since it makes clear what the main geological problem involved in planning the D-Day landings was.

Many of our own coasts and those of the French side of the Channel are characterised by off-shore stretches of a wave-cut platform of solid rock with here and there deep passages cut through it; then, landward of this, patches (often extensive) of plastic clay and peat overlain by a veneer of modern beach sand of variable thickness; and, finally, a storm beach of pebbles behind which the modern fen-like deposits are now forming, explained Professor King. These conditions worried our planning staffs, since experiments on counterpart British beaches had shown that most vehicles could not travel over outcrops of peat or plastic clay and were even liable to cut through a thin cover of sand and become bogged in the underlying soft deposits. It was known from French records that peats existed on the Normandy beaches but in the early days of the planning the beaches appeared from air photographs to be evenly covered by sand. After the storms of 1943, however, suspicious dark patches in the bottoms of the runnels suggested that peat might be exposed. Much careful work over the next six months culminated in the preparation of large-scale maps of the beach indicating those parts which were unsuitable for the passage of assault vehicles, and a pre-invasion reconnaissance by commando volunteers confirmed the existence of outcropping peat. The success of the landings was sufficient justification for the time spent on this problem. By far the greater part of the geological research was carried out by Professor Shotton, who was then geologist at H.Q. 21st Army Group.

The Design of Animals

The address of Dr. C. F. A. PANTIN, president of Section D (Zoology), was both scientifically stimulating and extremely well integrated so that it had the same kind of attraction as a work of art. His theme was "Organic Design", and this central design he picked out in colourful threads selected from the last hundred years of zoology; a period beginning with the work of Richard Owen, our greatest pre-Darwinian anatomist, only eight years after which—in the same year as the Prince Consort was British Association president—came the revolution of Darwin's evolutionary theory expressed in *Origin of Species*.

Pure morphology, concerned with the significance of organic form, was the dominant feature of Victorian zoology. After 1918 there was a revolt; functional studies, in which structure often seemed of little importance, filled the stage, but, said Professor Pantin, in the last 20 years almost every one of these has led back again to the importance of *structure*, though from a very different aspect from that of evolutionary morphology.

Professor Pantin traced how this came about. By the beginning of the nineteenth century, two basic principles were emerging from the study of comparative anatomy; *first, that structures were adapted to the function they served in the organism; second, that quite apart from function some plan underlies the architecture of whole groups of animals so that the same characteristics recur again and again in different species.*

The *Origin of Species* (1859) transformed the whole of biology: "Nothing is more remarkable in the history of science than the speed with which Darwin's work caused the theory of evolution to be generally accepted", said Professor Pantin. It first affected abstract comparative anatomy. Darwin's theory did not contradict the generalisations of the anatomists; on the contrary, it gave elegant explanations of them. Much the most important conclusion was that *the mechanistic operation of natural selection could produce purposive adaptation.* Purposiveness had supplied the teleologists' main argument in favour of an extra-physical or divine directive process governing form. Now it became, as it were, the negative image of blind

environmental forces. Other generalisations also received an explanation, at least in part. Under the stimulus applied by Darwin, the study of comparative anatomy made immense advances in the next 60 years. It dealt particularly with the hunt for ancestral types—the 'missing links' of the popular writers.

Physiological Similarities

After the appearance of that magnificent landmark, Sedgwick's *Text Book of Zoology*, zoologists turned to a dozen new lines of research far removed from traditional comparative anatomy—comparative physiology, genetics, animal behaviour, ecology, for example. This was a period of intense activity concerned with study of the *living* animal. The new outlook was vigorously mechanistic. There was a praiseworthy distrust of authority in favour of direct observations and concrete physical models in explanation of them. But the use of such models carried some danger of over-simplification, and the underlying assumptions of much of this work were often curious: particularly those concerning the cell. Bearing in mind all that evolutionary divergence implies, one realises that it was scarcely logical to suppose that the few known points of *structural similarity* between objects as different as vertebrate nerves, algae, amoebae and sea-urchin eggs would justify the assumption of far-reaching *physiological similarity*—"that experiments on the permeability of a species of amoeba could throw any light on the organisation of a sea-urchin's egg". Yet physiological similarities were found. For instance, some of the biochemical operations involved in alcohol production by yeast and in muscular contraction proved to be identical. Identical chemical machinery was being used by very different organisms for very different ends.

Biochemical similarity taken by itself provides, however, very weak evidence of evolutionary relationship. A sufficient number of biochemical parallels might relate *Chlamydomonas* more nearly to an oak tree than to the superficially similar *Euglena*; but the strength of the argument depends upon the number. Often, biochemical similarity cuts right across apparent evolutionary relationship. Thus in the leech and in the vertebrates there is similar sensitivity of the muscles to the peculiar chemical called *acetylcholine*.

"*There is a detailed parallel between the leech (Hirudo) and the Vertebrata in the mode and functional site of action not only of acetylcholine, but also of adrenalin, and of many pharmacologically active substances. Indeed such physiological similarities were among the evidence which led W. H. Gaskell (in his book "The involuntary nervous system", 1920) to put forward his heretical view that the Vertebrates were evolved from that off-shoot of the Annelids, the Arthropods. But in spite of all this there is overwhelming evidence that leeches and Vertebrates are utterly sundered in their evolution. We now know that acetylcholine is a common synaptic intermediary at some point or other in the nervous systems of almost all, but not quite all, multicellular animals; though the patterns of pharmacological action are rarely so alike as those of Hirudo and the Vertebrates happen to be,*" said Prof. Pantin.

Today it is known that acetylcholine is not involved solely with nervous transmission. This chemical is found in unicellular organisms, in bacteria and Protozoa (including *Paramoecium*, which also forms adrenalin). It also occurs in plants; it is responsible for the sting of stinging nettles. The fact that man and *Paramoecium* both have it does not necessarily argue for a common ancestry in the same way that the pentadactyl limb does for a man and for a mole. Whereas it would be surprising to find on another planet an animal with a pentadactyl limb, it would be less unexpected—"the laws of light, as of gravitation, being the same in Jupiter as here" and the properties of molecules also—if animals did happen to exist on that planet to find them possessing acetylcholine and even using it for the transmission of excitation. The possession of a particular chemical of this sort might possibly prove to be an inevitable necessity for living systems resembling ours. Whereas it seems highly improbable that the unique assemblage of genetic factors which ensures the development of a pentadactyl limb would ever be selected independently on two separate occasions.

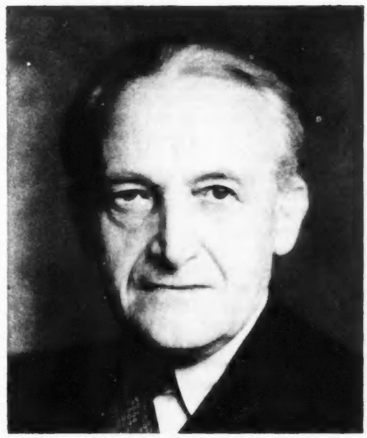
Prof. Pantin referred to the remarkable carbon chemistry of living organisms. Biochemistry, he said, suggests that organisms make the fullest possible use of the special properties of a very limited supply of suitable elements. Indeed it seems remarkable that the properties of the few kinds of elementary unit available should permit the construction of so improbable a structure as a living organism. Chemically the organism is built up of standard parts with unique properties.

Building with Meccano Sets

The older conceptions of evolutionary morphology stressed the graded adaptation of which the organism is capable, just as putty can be moulded to any desired shape. But biochemistry leads us rather to consider the organism as more like a model made from a child's engineering constructional set: a set consisting of standard parts with unique properties, of strips, plates and wheels which can be utilised for various functional objectives, such as cranes and locomotives. Such models can in certain respects show graded adaptability, when the form of the model depends on a great number of parts. But they also show certain severe limitations dependent on the restricted properties of the standard parts of the set.

Prof. Pantin used an analogy from bridge building to demonstrate the further principle that any functional problem must be met by one or other of a few possible kinds of solutions. In bridge building, it must be a suspension bridge, or a cantilever bridge, and so on. The engineer who constructs the bridge must choose whichever of these solutions he can best employ with the standard parts at his disposal. In the design of a bridge there are in fact three elements: the classes

SIR CLAUD GIBB
Engineering



PROF. C. A. MACE
Psychology

PROF. W. BROWN
Botany

possible in this universe, the unique properties of the materials available for its construction; and the engineer only takes third place by selecting the class of solution, and by utilising the properties of his materials to achieve the job in hand. He is in a sense merely executing one of a set of blue-prints already in abstract existence; though it requires insight to see that the blue-print is there.

The living organism, like all material structures, must conform to certain constructional principles. And the blue-prints of many of these may be said to be shown to us in that magnificent work of D'Arcy Thompson, *On Growth and Form*. The standard parts available for the construction of organisms are the units of matter and energy which can exist only in certain possible configurations. Like the engineer natural selection takes third place by giving reality to one or other of a series of possible structural solutions with the materials available. But the fact remains that we have arrived back at the eighteenth-century conception of an ideal plan as an essential constituent of organic design. Yet any such plan is not peculiar to living things; it concerns the whole inanimate and animate universe.

The same limitation involved in the use of standard parts with unique properties is in force not only at the molecular level of structure; it also affects all higher levels of structure.

Prof. Pantin spoke of what he termed *emergent properties*—the new properties that emerge when elementary components are assembled together. Thus four wheels, two spindles and a plate from the constructional set gives a truck, with quite new properties. At the molecular level, Prof. Pantin cited the following analogy from biochemistry. Living organisms have few ways in which to form fibres; one way involves the long molecules of fibrous proteins which form flexible threads and membranes. Cross-linkages, like those introduced when rubber is vulcanised, toughen these materials; this very method of sulphur linkage is used to make horn, nails and hair. Tanning is another way of hardening protein; the skeleton of insects is hardened by tanning—it happens too with the egg-shell of liver flukes, in the silk of the silk-worm and of the stickle-back's nest, and in the tough guy-ropes by which the mussel clings to rocks. All sorts of animals independently utilise the emergent properties of this system for different purposes.

Long protein fibres are capable of *contracting*. The essential character of muscular contraction seems to remain the same in every kind of animal, and it is indeed extraordinary that the wing muscles of a minute fly *Forcipomya* which execute a contraction in a thousandth of a second may utilise essentially the same machinery as a lazy sea-anemone, some of whose muscles take six minutes to contract. For an engineer to vary speed of movement well over a hundred thousand fold would involve several changes of mechanical principle. Again, muscle fibres able to develop tension quickly without great change of length are invariably striated—indeed similar striation occurs even in the organs in protozoa (e.g. *Stentor*) which correspond to muscles. Whatever the physical basis of striation the repeated independent evolution of this complex structure leads one to suppose that it arises from some easily utilised feature of contractile protein threads.

At a higher level of structure come such organs as the organs stimulated by light. The possible kinds of stimuli an organism can receive are few and highly specific. Of these, light rays are the most easily employed for the detection of pattern in the external world. But to record the pattern of light stimuli there are only a few possible kinds of morphological mechanism. There is the *lenticular eye*, which functions like a photographic camera. There are one or two kinds of *ommatidial eye*. [This kind of eye occurs in insects, crustaceans, etc.] There are also possible instruments based on a process of '*scanning*' the environment with a single receptor in a manner somewhat analogous to the process of scanning used in television; this is essentially what happens in the flagellate *Euglena viridis*. Research has shown that the demand for patterned sensory information can be met only by a few highly specific designs from which the animal must choose, if it is to achieve the highly advantageous goal of purposive behaviour. Hence, given the common properties of the limited materials available, inevitably the same kind of complex sense organ must be independently evolved again and again. "In this sense, and I believe in this sense only, is Bergson right. There is no need for a vitalistic drive to achieve an eye. But a lenticular camera is an inevitable class of sensory instrument both for animals and engineers," commented Professor Pantin.

The Incredible Insects

The evolution of the various mechanisms enabling an animal to integrate present and past stimuli and to predict a suitable line of action is equally interesting. Here the same end can be achieved by the utilisation of different means; thus the behaviour of the unicellular Protozoa which essentially resembles that of Man is not dependent on nerves, or synapses or complex cellular sense organs. The warning that it is dangerous to suppose that each functional problem can be met by one and only one possible mechanism—is also particularly applicable to insect behaviour. The staggering complexity of behaviour of which von Frisch has shown the honey-bee to be capable is all operated through a brain weighing about 2½ mgms.: close on a millionth part of our central nervous system.

As Darwin said, "*It is certain that there may be extraordinary mental activity with an extremely small absolute mass of nervous matter: thus the wonderfully diversified instincts, mental powers, and affections of ants are notorious, yet their cerebral ganglia are not so large as the quarter of a small pin's head. Under this point of view, the brain of an ant is one of the most marvellous atoms of matter in the world, perhaps more so than the brain of man.*"

Professor Pantin concluded with these words: "Our conclusion is not unlike that of Owen; that there are two principles governing form. Unlike him, we attribute one of these to natural selection, but like him we recognise in the other an abstract plan. But this plan is not peculiar to living things. There is nothing vitalist about it. It emerges from the unique properties of matter and energy, and even its more complex consequences govern the constitution of inanimate objects like calculating machines, as well as living systems.

"I want you to notice one thing more. We have been searching for relationships of one kind or another governing form. It is by discerning relationships that science advances. But let us also notice that the faculty by which we do this is aesthetic. The very great advance of biology in a hundred years owes not a little to our grandfathers' appreciation of the amazing beauty of the natural world."

100 Years of Geography

THE geographers listened to an address by their president, Dr. O. J. R. HOWARTH (the immediate ex-secretary of the British Association) on the centenary of their section—Section E.

Geography (including Ethnography) became a separate section at the Ipswich meeting of 1851. Previously it had been an appendage of Geology. Fifteen years later ethnology transferred to the Biology Section.

Dr. Howarth's lecture was largely concerned with the praise of 'famous men', starting with Sir Roderick Impey Murchison (1792–1871), who for 20 years was the main driving force of the section.

Readers will find the *Who's Who* at the end of this paper extremely useful. It includes such names as Candolle, Darwin, and Wallace, Admiral Fitzroy (commander of H.M.S. *Beagle* during Darwin's voyage and a pioneer of official meteorology), Galton, Sir Patrick Geddes, Sir Archibald Geikie, Prince Kropotkin, Penck, Scoresby, Scott, Shackleton, Sir H. M. Stanley, and Whymper. All these men, together with the inseparable trio Herbertson, Dickson, and Mackinder (the leading British exponent of geopolitics, whose work has received recent attention in DISCOVERY, *see* E. M. Friedwald, 1947), have been important in the history of Section E.

The Nature of Profit

One of the sectional presidential addresses which defies summarising was that to Economics (Section F) by Professor R. G. HAWTREY on "The Nature of Profit". It is impossible to do more than quote a few of his remarks.

Profit-making, he said, had long been under attack; but he pointed out that proposals for ridding society of the abuses of profit-making should make some alternative provision for the services the profit-makers rendered. Those who favoured nationalisation, whether of all industry or of selected industries, were rather apt to assume that the State in acquiring an industry took responsibility only for the

technical business of production. That was too limited a view. The producer took the responsibility for understanding and suiting the needs to be met by his product. For private enterprise the test of success in doing so was the sale of the product. Enterprise really meant enterprise in meeting a demand.

There was, the president thought, a danger that nationalised enterprise would take the easy road of imposing standardised products on the cónsumer and would blindly obliterate refinements by which material welfare was raised above an elementary minimum. When public policy intervened in industry, whether by nationalisation or planning, it was inclined to favour standardisation and mass production because those methods of production contributed to a war potential.

When the big incomes of profit-making were attacked not by nationalisation but by direct taxation the danger was not so much in the impairment of incentive as in the adverse effect upon saving. Profits, said Professor Hawtrey, are the principal source of saving.

An Engineer's Prophecy

The president of Section G (Engineering) was SIR CLAUDE GIBB, who was even more ambitious in his historical survey and covered not a century but 2000 years of engineering. Towards the end of his paper he indulged in speculation about the future, saying for instance, that within the next fifty years the gas turbine-driven motor-car will be used to the exclusion of all others. For very large powers, the steam turbine would not be ousted for some time, but he saw no obstacle to the making of gas turbine units of 100 h.p. for fitting to motor-cars.

Some of the other possibilities he foresaw were: vast schemes of electrification for railways, the result of producing electricity by nuclear energy; the abolition of the open coal fire; high-pressure gas mains supplying heat to home and industry; all aircraft powered by jet engines; gas turbines replacing diesel engines; electric pump-produced rain (i.e. overhead irrigation) and electrically heated soil which could help to solve our food shortage problems.

Of the production of electrical energy from fissile material, he said: "With the prevailing costs of coal and petroleum and the present-day cost of uranium, it is doubtful whether electricity could be produced at the moment more economically in this way. Yet it requires only a small improvement in the method of using the heat generated by fission to enable atomic energy to compete with present-day practice."

He said that ultrasonics would become an everyday thing in our industrial, domestic, and medical life.

Work was now being undertaken in this country in burning methane, which occurred in small proportions in the large volumes of air used in mine ventilation. It might be that in the future by using a high degree of pre-heat, before combustion, this—the miner's greatest enemy, fire-damp—would be used for the benefit of man.

He concluded: "When one considers the progress in engineering and the contribution to living standards in consequence which the century 1851–1951 has seen, he would be a brave or a foolish man who would put a limit to prophecies for the future."

The address to Section H (Anthropology/Archaeology) by SIR CYRIL FOX was a paper for specialists, being a survey of research into early Celtic metalwork in Britain. He considered that plenty of scope remains here for further research into the evolutionary progress of patterns. He suggested that an index of Celtic art ought to be prepared. He hoped this idea would appeal to a university, or to one or other of the institutions such as the Courtauld Institute in this country which could fittingly organise and maintain such an index.

The Physiology of Capillaries

Another specialist address was that given by Professor H. P. GILDING to Section I (Physiology). His subject was the physiology of the capillaries.

People, he said, had thought of these tubes—the capillaries—with their exquisitely thin walls (less than one micron) as conveniently placed channels through which filtration could occur, and tended to forget that they were made up of living cells with a membrane, or rather two membranes, through which substances must pass.

Should we not get further in our understanding if we regarded those cells and their permeability as being subject to the same physico-chemical laws as other cells in the body—remembering that internally they were exposed to a small hydrostatic pressure and that their inner and outer membranes were much closer together than most other cells, and further, in common with other living cells, could alter their permeability to meet the requirements of the moment? Might it not be also that the endothelial cell, normally placed to have the first pick at any available oxygen, was unable to carry out and control its function of tissue exchange in circumstances when production of energy was depressed?

Professor W. BROWN, president of Section K (Botany), reviewed progress in mycology and bacteriology over the past century. He began by reminding his audience that it was only within that period that the theory of spontaneous generation was given its quietus. Thus in the 1830s it was stated that "Fungi are not plants at all; they are *lusus naturae*". In the controversy which centred round the spontaneous generation of small organisms, some of the chief supporters were leading chemists of their day. Berzelius was one of them; another was Liebig, who died in 1873 and who maintained it all his life. When Cagniard Latour and Schwann (author of the Cell Theory) independently and almost simultaneously (1836–7) claimed that yeasts or bacteria brought about the process of fermentation in sugar solutions the chemists ridiculed the idea in no uncertain terms and in a manner that was far from polite. They asserted that fermentation was a purely chemical reaction, caused by an unstable ferment which communicated its instability to certain organic substances. This in fact is true in a kind of way, but the ferment is derived from the micro-organism. It was not till sixty years later that this ferment was prepared from yeast and its properties studied.

Some justification for the chemists' opposition to the biologists' theory could be found in the establishment by Wöhler in 1828 that no 'vital force' was necessary to build an organic substance like urea. The upshot was a tendency to discount the existence of any real barrier between the organised and unorganised states of matter. In the light of this trend in chemical thought, it did not seem unduly optimistic to suggest that organic material could regroup itself and become organised, i.e. become living substance.

It was the classic researches of Pasteur which established the biological theory of fermentation, and which killed the theory of spontaneous generation as applied to fungi and bacteria. His studies (1857) on lactic acid fermentation were followed six years later by equally significant work on butyric acid fermentation. (Incidentally, this is the first account in literature of an anaerobic type of organism: Pasteur invented the term 'anaerobe'.) The same views had been put forward thirty years earlier, but they did not gain acceptance, either because they were not established beyond criticism, or because the timing was not ripe for the new idea—probably the latter. Pasteur's experimental genius settled the matter beyond all doubt and brought the scientific world round to his way of thinking. It is perfectly right therefore to regard him as the founder of microbiological science.

Pasteur proceeded to apply his techniques to disease-producing micro-organisms, and these researches led to one of the greatest contributions by science to the benefit of mankind. Contemporary bacteriologists were Robert Koch and Ferdinand Cohn, the founders of bacteriological technique and systematic bacteriology respectively. In mycology the great names were the Tulasne brothers in France, and the Franco-German Anton de Bary.

Systematic mycology had been founded by Elias Fries, whose *Systema Mycologicum* was published over the years 1821–9. He was very much at home with the larger fungi, but such things as the Rust and Smut fungi were beyond his ken—he saw their study flourishing "to the detriment of the knowledge of the nobler fungi", adding that "all study of them seems to me to be physiological frustration". Fries was, moreover, a believer in spontaneous generation.

Then in 1861–5 appeared the *Selecta Fungorum Carpologia* of the Tulasne brothers, who with de Bary were laying the foundations of a new mycology with their pioneer investigations of fungal life-histories. De Bary's *Comparative Morphology and Physiology of Fungi, Lichens and Myxomycetes* is probably the most notable book in mycological history.

The development of methods for obtaining pure cultures of fungi and bacteria

took time. A major difficulty was that of sterilising nutrient media; sometimes ordinary boiling worked, sometimes not. In 1876 Cohn showed that the spores of *Bacillus subtilis* could resist heat. To kill such organisms John Tyndall, the physicist, devised his method of *discontinuous sterilisation*—in this method the nutrient medium is boiled once to destroy the living bacteria, and a second time to destroy the new crop of bacteria that results from the germination of the spores (which are unaffected by the first boiling). Then came the introduction of the *dry cottonwool plug* to protect pure cultures of fungi and bacteria and sterilised media contained in flasks and test-tubes—the plug allows entry of air needed by the respiring organism, but filters out all organisms that might cause contamination of the cultures. In the 1880s the gelatine or agar plate and the familiar Petri dish were devised in Koch's laboratory.

Cytology at the nuclear level had to develop before the life-histories of Ascomycetes and Basidiomycetes could be thoroughly understood, sexuality in these fungi being largely or entirely an affair of nuclei.

Professor Brown concluded his review of the advance of fungal taxonomy by remarking on a striking—"I might say a terrifying"—feature of it, and that is the enormous number of species which have been recorded and named. There are more than 80,000 in the pages of Saccardo, and many have been added since.

An added complication is the existence of *strains* within a species. Perhaps the most startling illustration of this complexity is given by the rust fungus *Puccinia graminis* of which over 100 strains are known, all of them occurring naturally. Here the strains differ only slightly from each other in morphological features, the important differences being physiological, whence the term 'biological race'. With other species morphological as well as physiological differences may also be shown, as in the strains of *Penicillium notatum* which have been studied in connexion with the production of penicillin. This feature of fungi is so common as to be almost universal.

Fungi are subject to the same genetical laws as are other plants, and within the species they show a similar type of variability which rests on genetic factors. By a process of crossing, new groupings of characters can be produced. Hence the possibility, and in fact the actuality, of improved races of fungi for economic purposes, as has been recently shown with strains of yeast for brewing and of *Penicillium* for penicillin production. There are also dangerous possibilities, as in the appearance of more virulent strains of pathogenic organisms, either by mutation or by natural hybridisation, or according to some workers, by a chemical process of adaptation. Examples which illustrate this danger are numerous and continue to increase; two such of recent origin are the strains of the potato blight fungus which are able to attack varieties of the host which were hitherto immune and the strains of *Streptococcus* which are resistant to penicillin.

A Mycological Gold Rush

Professor Brown referred to the "mycological gold rush" which followed the discovery of penicillin. This discovery prompted a feverish exploration of the whole range of fungi for new types of 'antibiotics'. The finds, though of the greatest value, have been relatively few, but it does not follow that this field of inquiry has been exhausted. The more thorough investigations which are now going on may lead to the discovery of other valuable compounds, and incidentally they throw new light on the processes of fungal metabolism.

The speaker mentioned the symbiotic nature of lichens—which are composite organisms, each containing a fungus and an alga. He said that there is much scope for research into problems of lichen physiology.

In the study of the physiology of fungi, the trend would be towards more and more biochemistry. "Many of us, who are not too firmly grounded in biochemistry, may feel uneasy at this prospect, may feel that we are handing away our subject to strangers. But I do not think that we should take too humble a view of ourselves. After all it is the biologist who poses the exact problem for the biochemist. He says in effect that there is such and such a process taking place and that in all probability there is a chemical substance concerned in it. What is the substance and what are its properties? Granted an answer, the problem again reverts to the biologist, whose function it is to apply the new information to a deeper understanding of the biological process," said Professor Brown.

"*This co-operation will be made easier and more fruitful if biological training is orientated more in a physico-chemical direction, especially in view of the rapid advances which are taking place these days in fundamental aspects of physics and chemistry. Considered in this light, the recent change in the schools whereby a certain amount of biological instruction has been substituted for further training in physics and chemistry is unfortunate. It is all to the good that the ordinary citizen should be equipped with some general ideas on biological matters; to the student who aims at making a career in biological research, the change is a retrograde one, for it cuts him off prematurely from the kind of training which, highly desirable as it has been in the past, appears likely to be essential in the future.*"

Science and the Layman: A Psychologist's Opinion

Prof. C. A. Mace, in his presidential address to the Psychology Section, made an eloquent appeal for more co-operation between psychologists and the laity. In the course of tracing some of the influences from which modern British psychology had developed he emphasised the human qualities that had been so often combined in eminent pioneers. Francis Galton, for example, who established the first psychological laboratory in Great Britain and laid down the basic concepts of mental testing—he even devised a method for the measurement of boredom by counting the number of fidgets per unit of time—could be described as a human naturalist, for he studied men in the same sort of way in which Gilbert White studied bees and field-mice. C. S. Myers, a humane and cultured scientist who played the violin, wrote a textbook of experimental psychology, and founded an institute for the application of psychology to industry, moved in circles that ignored the chasms dividing the art, the science, and the technology. Prof. Mace said it is one of the tasks of psychologists to carry on this tradition of humanism and to halt the progressive divergency between artists, scientists, and technologists.

As a corollary to this, Prof. Mace made probably the first case ever presented by a leading psychologist for the popularisation of science. He said: "First, psychologists have a special responsibility for the maintenance of the British tradition in the popularisation of science. No apology is needed for the use of this word. It is in this tradition not only to maintain the highest standard of scientific journalism, but also for those who themselves make discoveries themselves to present them to the laity. A popular science series published in the nineteenth century includes among its authors Huxley, Spencer, Lubbock and other distinguished scientists. These names recur in the [magazine] *Nineteenth Century* itself. It was a British scientist who said that he never felt quite satisfied with a theorem until he felt that he could explain it to the next man he met in the street. It is in virtue of this tradition that the British Association is a *British* association—an association of professionals and laymen."

The second corollary is on similar lines to the first. Psychologists, according to Prof. Mace, have a special responsibility for the preservation of a common medium of communication. Technical terminology may be used as an instrument of power politics, as a code, and an instrument of exclusion by the members of a cult. This is a free country and scientists must be allowed to talk to one another in whatever language they like. They must be allowed to use what words they like, so long as they tell us what they mean. They must, however, try to be bilingual. In the case of psychology Prof. Mace did not believe the demand to be exacting. He had not met a psychological fact or a psychological theory that could not be expressed in language which an intelligent schoolboy—of up to School Certificate standard—could readily understand. Psychologists . . . write no prescriptions and they have no secret remedies. All that they know they must tell. It is not for them to lay down rules but to communicate understanding, first of all to parents, then to teachers, and then to those concerned in any way with the management of men. The task is one for humane, cultured scientists, and a humane cultured laity.

This report of the Edinburgh meeting of the British Association will be concluded in the November issue.

Far and Near

Night Sky in October

The Moon.—New moon occurs on Oct. 1d 01h 57m, U.T., and full moon on Oct. 15d 00h 51m. The following conjunctions with the moon take place:

October				
14d 04h	Jupiter in conjunction with the moon	Jupiter	5°	S.
26d 17h	Mars ,,	Mars	2°	N.
26d 23h	Venus ,,	Venus	0·04°	S.
28d 18h	Saturn ,,	Saturn	5°	N.

In addition to these conjunctions with the moon, Mars is in conjunction with Regulus on Oct. 3d 03h, Mars being 0·9° N.

The Planets.—Mercury rises nearly an hour before the sun on Oct. 1 and is visible for a very short period in the eastern sky but will be a difficult object to find. The planet is in superior conjunction (the earth, sun and Mercury are then in a line) on Oct. 13 and is too close to the sun for the remainder of the month to be seen. Venus is a morning star, rising at 3h 10m, 2h 40m, and 2h 35m, on Oct. 1, 15, and 31, respectively. The stellar magnitude of the planet is —4·3 and the visible portion of the illuminated disk varies between 0·20 and 0·42. Mars, close to Regulus on Oct. 3, rises soon after 2h during the month. Jupiter is in opposition on Oct. 3 and early in the month rises about the same time as the sun sets and sets about the time of sunrise, and is visible throughout the night during the month, stellar magnitude —3·5. Saturn is too close to the sun in the early part of October to be seen but from about the middle of the month is visible, rising at 5h and 4h 10m on Oct. 15 and 31, respectively. Towards the end of the month the planet is very close to γ Virginis.

"Endeavour" Prizes

THIS year the magazine *Endeavour* held its second competition for scientific essays—the aim of which is the encouragement of better writing by science students—and the prizes were presented by the Duke of Edinburgh on August 9 at Edinburgh.

The first prize (50 guineas) went to 23-year-old B. C. KILKENNY, who is engaged in postgraduate research at Oxford, for an essay on biological effects of radiation. Second prize was awarded to J. H. PANTIN, 21-year-old son of Prof. C. F. A. Pantin. He is an Oxford undergraduate. The winner of the third prize was P. G. GARNER of King's College, London.

The special prize of 5 guineas, for entrants under 18, was given to 17-year-old M. J. SHERIDAN of Hornsey County School.

The National Radio Exhibition

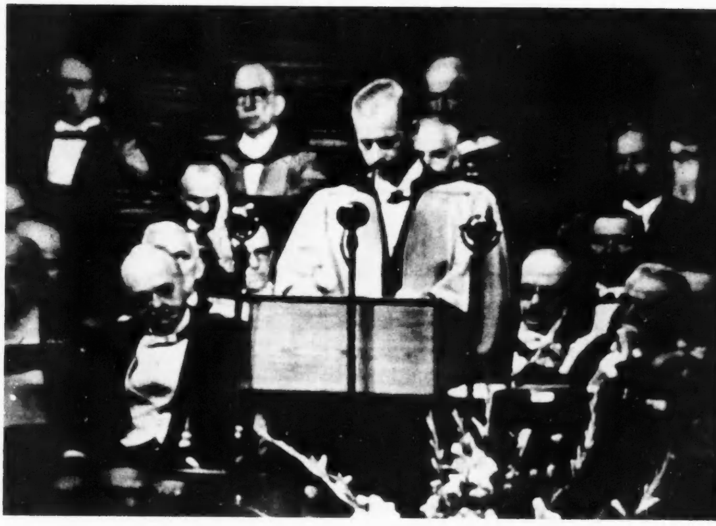
VISITORS to the Radio Exhibition this year found themselves in the unfamiliar surroundings of Earls Court instead of the usual Olympia exhibition hall. As a result, one B.B.C. announcer on duty at the exhibition went to the wrong hall and taxied to Earls Court with only a few minutes to spare.

Once inside, visitors and exhibitors agreed that the change of venue was a great improvement. The Earls Court hall is more airy, and it allowed more spacing between the stands; the overhead lighting is good, and at no time does one get any sense of the heavy atmosphere which sometimes seems to come over an exhibition. Nevertheless the attendance was not up to expectations, probably because there was little novelty in the manufacturers' exhibits and no particularly outstanding attraction. The increased purchase tax on receivers must inevitably deter many from buying an improved model, and on the manufacturing side there was little evidence of economy in design or construction to save a few shillings. On the whole, the new models show a gratifying tendency to couple excellent musical quality with good design of the cabinet and no fancy additions.

Among the features of the designs are those that have been described in technical publications during the past few years: speakers with high flux-density magnets, acoustically designed cabinets, volume controls with tone compensation, and negative feedback circuits. The majority of radio-gramophones are now fitted with three- or two-speed motors for long-playing records, but no newcomers have entered in the record field since last year. Many technically interested visitors are weighing the merits of a tape recorder against the long-playing record, and a number of well-made tape recorders and reproducers were available at prices comparable with the radio-gramophone outfit.

The imminent opening of the new television station at Holme Moss was reflected in a number of television receivers which could be adapted to either the London or North Country wavelength, the modification being carried out by the dealer. Television was again the main attraction, and the excellent arrangements by the B.B.C. enabled crowds of visitors to see a 'live' broadcast being televised from the studio erected in the hall.

A notable increase in the number of projection receivers was seen this year, although in some cases the picture suffered by comparison with a direct-viewing screen which was injudiciously placed alongside it. The 12-inch direct-viewing cathode-ray tube is a favourite with manufacturers and public, as an analysis by the *Wireless Trader* has shown. Of a number of receivers counted in the exhibition, 6·7% had tubes of 9–10-inch diameter, 66% had 12-inch tubes, and 11% were projection types. It is probable that the coarser 'grain' of the projection picture will not be in its favour for viewers with small and medium-sized living-rooms where it is difficult to arrange the furniture to obtain the necessary viewing distance. One advantage of the projection receiver which did not seem to be adequately stressed is its application to school broadcasts and demonstrations to fairly large audiences. The success of



Large-screen television was on view at the National Radio Exhibition. It was also demonstrated impressively at the Edinburgh meeting of the British Association. Thanks to Sir Edward Appleton's initiative, television enabled an overflow meeting of 2000 people in Usher Hall to witness the inaugural ceremony in McEwan Hall half a mile away. The picture, on 16 × 12 ft. silver screen, was remarkably clear, and one paper commented that it gave the overflow audience a more intimate association with the proceedings than was obtained by those actually present. This is a photograph of the TV picture of the Duke of Edinburgh. (*Photo by "The Scotsman".*)

television as a teaching aid has already been shown in the medical schools, and the recent televising of the Duke of Edinburgh's speech at the B.A. Meeting (this was organised by Sir Edward Appleton) showed the large-screen projection receiver at its best.

A high proportion of cathode-ray tubes are now fitted with aluminised screens or ion traps, and the high tension supply to the tube is steadily increasing in magnitude. A new metal-glass tube manufactured by the English Electric Co. requires 12,000 volts for normal operation. It has a screen diameter of 15 inches and an exceptionally wide angle of deflection of the beam. Tubes of 12-inch diameter with rectangular screens were also shown.

A novel feature shown on the Amplion stand was the 'Actibette' battery renovator, which is claimed to prolong the life of dry batteries like hearing-aid batteries up to six times the normal duration. It does not, of course, renew batteries which are 'dead', and it is recommended that batteries in service should be connected to the renovator periodically for as long a time as they have been in active use.

The DSIR showed on their stand the use of ionisphere recording in short-wave communication. A second display showed the types of weather which affect short-wave reception, and how the optimum wavelength can be predicted for a given set of conditions.

Among the more entertaining exhibits were radio-controlled boats, a scale model of the London Airport radar control system, a motor boat fitted with radar and radio-communication equipment, and an eight-channel electroencephalograph.

A final anecdote which gives rise to reflection on the advantages of advertising: Mr. E. K. Cole, who has spent many tens of thousands of pounds in publicising his own (and his firm's) name, was photographed on his stand by a Press cameraman. The exposure made, the photographer inquired the name of the subject "That's Mr. Cole himself," was the reply. "What initials?" asked the photographer.

International Computation Centre Planned

EXPERTS from thirteen countries have been discussing at Unesco House in Paris plans for the establishment of an International Computation Centre. This centre will be the first United Nations research laboratory.

As long ago as 1946 the Economic and Social Council commissioned a general report on the problem of establishing United Nations research laboratories. This was prepared, and in August 1949 a committee of experts from Unesco and United Nations examined it, and recommended the following priorities in the natural sciences. *First priority*: International Computation Centre; International Brain Institute; *second priority*: International Astronomical Laboratory; International Institute of Biochemistry; International Meteorological Institute; International Research Laboratory on Arid Zones. A year later an Economic and Social Council resolution invited Unesco to prepare a detailed plan for the setting up of the proposed Computation Centre.

150 Years of Submarines

IN 1801 the first real submarine—the *Nautilus*, designed by ROBERT FULTON (1765–1815)—was tried out in the river Seine and in the sea off Brest. The idea of a submarine was, however, too revolutionary to find acceptance, and a long time was to elapse before any navy adopted it. Both sides in the American Civil War had submarines, though they did not use them effectively. It was not until electricity for under-water propulsion became available that the submarine underwent any intensive development; Britain became interested about 1900, and the story of submarine development by the Germans and the use they made of it will be familiar to all our readers.

Fulton's greatest claim to fame rests on his construction of the paddle steamer, the *Clermont*, which travelled in 1807 from New York to Albany, a journey of 150 miles which took 32 hours.

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